Gravitation versus Relativity

Charles Lane Poor

Ether Rock, Mount Wilson Observatory, California, with Interferometer House, used by Miller in 1921.

Here, at an elevation of 6000 feet, the Michelson-Morley experiment was repeated, and the results indicated a true, though relatively small, ether-drift. If these results be confirmed, then the very basis of the Relativity Theory will be destroyed and the whole Einstein structure will collapse.

Gravitation versus Relativity

A Non-Technical Explanation of the Fundamental Principles of Gravitational Astronomy and a Critical Examination of the Astronomical Evidence Cited as Proof of the Generalized Theory of Relativity

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With a Preliminary Essay by

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AUTHOR'S PREFACE

This work is intended for the general reader as well as for the scientist, working in lines other than astronomy. It is an attempt to explain in non-technical language and without the use of complicated mathematical formulas the fundamental facts and principles of gravitational astronomy, and to submit the so-called astronomical proofs of the Relativity Theory to a critical examination and discussion. The validity of these proofs cannot be passed upon by one who is totally unfamiliar with the facts and methods of astronomical research. It is not, however, necessary for one to know all the complicated details of planetary motion, nor to be familiar with all the methods of determining the size, shape, and motions of Mercury, but it is essential for one, who wishes fairly to judge the evidence, to know the fundamental methods and approximations used in determining the motion of a planet about the sun. To unfamiliarity with these methods may be traced a widespread misconception as to the intrinsic value of the evidence.

Further, in the way in which this evidence has been presented and accepted, there has been apparently a complete reversal of ordinary scientific methods. As a new theory, as an hypothesis seeking acceptance, it

would seem that the burden of proof should rest upon Relativity; that its advocates should conclusively prove the necessity and the sufficiency of their hypothesis. Such, at least, has been the accepted scientific method in the past. In 1665 Isaac Newton developed his law of gravitation and put it to test in the motion of the moon. He found a minute discrepancy between his theory and the actual motion of the moon, a trifle less than one one-hundred-and-thirtieth (1/130th) of an inch in a second of time; a difference of about 15% of the observed motion. This small discordance caused Newton to consider his theory as *not proved,* and he laid aside his work. Nearly twenty years later new measurements of the earth were made, and, urged on by Halley, Newton corrected his older calculations and showed that his law of gravitation was substantially correct. Then, and then only, did he announce his theories. It was some twenty years after Charles Darwin first conceived his theory of evolution before he made it public in his classic work.

The theory of relativity, on the contrary, was announced without any confirmation. Tests were proposed, selected by its author, and these tests failed of confirmation by 20%, by 50% even, and yet such results are called thoroughly satisfactory. The merest indication of a result, favorable to relativity, becomes conclusive proof; and observations and experiments, which can be explained by the new hypothesis almost

as well as by the older methods, become crucial tests in favor of relativity. And the theory has been accepted, and is accepted by mathematicians, by physicists, by many of the most prominent astronomers of the world, and the burden of proof has been shifted, until it seems that relativity is an established scientific fact, unless it can be completely disproved.

For some years now the entire world has been in a state of unrest; mental as well as physical. The physical aspects of this unrest, the strikes, the socialistic uprisings, the war, are vivid memories; the deep mental disturbances are evidenced by the widespread interest in social problems, by the futuristic movements in art, by the light and easy way in which many cast aside the well tested theories of finance and government in favor of radical and untried experiments. Can it be that the same spirit of unrest has invaded science? The Relativity Theory, as announced by Einstein, shatters our fundamental ideas in regard to space and time, destroys the basis upon which has been built the entire edifice of modern science, and substitutes a nebulous conception of varying standards and shifting unrealities. And this radical, this destroying theory has been accepted as lightly and as easily as one accepts a correction to the estimated height of a mountain in Asia, or to the source of a river in equatorial Africa.

The bases of our fundamental concepts of time and space, and the psychological phases of the Relativity

Theory are but lightly touched on in this work, and then only when necessary to the clear presentation of the main subject. But these aspects of the theory have been most ably presented in the PRELIMINARY ESSAY by Professor Chamberlin, where the gradual evolution of our inherited fundamental concepts is traced through the eons of geological time, and the solid bases of these concepts contrasted with the shadowy structure of Minkowski and Einstein.

It is the main purpose of this book to present to the jury of the thinking world the concrete astronomical evidence cited by Einstein and the relativitists as proof of the Generalized Theory of Relativity, and to subject that evidence to a critical examination. Many a well-built-up case has completely collapsed under a searching examination of the evidence and a cross-examination of its chief witnesses.

The thanks of the author are due to Dr. George E. Hale, Director of the Mount Wilson Observatory, to Professor Dayton C. Miller, of the Case School of Applied Science, and to others for furnishing original photographs and drawings to illustrate the text; to Professors Bergen Davis and Harold W. Webb of Columbia University; to Mr. William E. Spandow, who read and corrected the text and compiled the index.

C. L. P.

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A PRELIMINARY ESSAY UPON THE FUNDAMENTAL CONCEPTS OF TIME AND SPACE

At no stage of history has equipoise in thinking been more vital to the welfare of mankind than at the present time. To a degree perhaps never equalled before, the social, religious, political, economic, and most other maxims that have served as guides in the past are receiving scant deference and are often suffering open question or active hostility. With this there has come a general loosening of restraints and an unprecedented venturesomeness into untried lines of thought, feeling and action. While all this is to be viewed with steady vision and philosophic calm—because beyond question there is in it much that is good as well as much that is bad—its import is so grave as to call for serious consideration. Millions have already perished unnecessarily; many more millions have suffered needlessly; still other millions are on the brink of calamity. On the other hand, perhaps even greater multitudes are rising—in spite of the turmoil—to higher planes of intellectual and ethical action. It is no part of the function of this little essay to balance the ledger of good and ill; that would be a formidable undertaking. It is merely its privilege to try to veer the trend of thought a little in the direction of restraint and circumspection.

The present loosening of ties and venturesome drift is not confined to the strenuous affairs of life just now distraught by extraordinary conditions; it reaches down into the fundamentals of thought and touches the intellectual instincts inherited from the great past. In particular, the basal concepts of space and time, the very framework of thought, are being called in question. Space has commonly been pictured as an unbounded receptacle for all that is and all that takes place, and time, as the tally sheet of the onsweep of an active world. Free room for the great deployments of the cosmos have been thought to be offered by unlimited space and ample duration for their evolutions in unrestricted time. The world's chief interest has always lain in the entities acting in space and time rather than in space and time themselves, but these have been felt to be none the less vital as necessary conditions for the work of the positive agencies. We are now asked to cast these great basal concepts aside and view space and time as dependencies tied up with one another and with the very entities and activities heretofore pictured as playing their parts within them. This seems to transform the whole into an intertanglement of variables and relativities devoid of a stable groundwork of inherent realities. Thus the very roots of the thinking of the ages are being digged about with little show of care whether it will promote growth or lead to withering. Specifically, it is affirmed that space and time, far from being boundless and independent, are so bound to each other that they have no independent existence, that all motion is merely relative, that there

is no absolute motion, that our measuring-rods are variable according as they move in one dimension or another, and the rate of our time-keepers faster or slower according as they move at one velocity or another. It is thus insisted that the very elements of thought need reconstruction. It is urged that the geometry of Euclid, the dynamics of Galileo, and the celestial mechanics of Newton are basally defective.

All these revolutionary claims are put forth, of course, in the name of progress. No doubt progress will follow this unsettlement of ideas when a new settlement shall be reached, whatever it shall be, but loss and damage are also liable to attend such a disturbance and reorganization. The vital question of the hour is how to preserve inherited values and add to them the greatest possible measure of new values, with the least possible adventure into what is futile or harmful. The world has reason to be proud of its recent advances in knowledge but it cannot blink the fact that there has also been an increase in ways of doing wrong and thinking foolishly. Quite assuredly the world knows much more now than in the days of Aristotle, but it is equally certain that it knows many more ways of making mistakes. Still, in the face of all the menacing entanglements of good and ill, it seems clearly the part of wisdom to push ahead, but it seems quite as clearly the part of folly to plunge into the untried without forethought and restraint. The law of wisdom is *to test first,* and put into the foundations afterward.

The great thinking public is not so much concerned with the ultra-refined accuracy or inaccuracy of partic-

ular systems of geometry, mechanics or dynamics as held by the great men of the past, as with the *soundness or unsoundness of the fundamental modes of thought inherited from the past.* Thinking men are profoundly interested in the question whether critical inspection today shows that the foundation stones of the intellectual structures thus far built are solid in substance and essence—though quite certainly affected by infelicities in selection, cutting, trimming and fitting—or whether such inspection shows that flaws and fissures seriously weaken the foundation stones and require their replacement before any higher superstructures are built upon them.

The first and most basal question therefore concerns the way these modes of thought have come into being and what sanctions they bring because of this origin. Under the evolutionary view, our basal modes of thought have grown up by test and trial out of the crucial experiences of the long past; not from human experience alone but from the tests and trials of the long line of living beings that formed the human ancestry. Out of these experiences have come the instinctive reactions that guard our physical welfare and the mental reactions once called self-evident or axiomatic truths. The question then follows: If the mental processes of the thinking and feeling world are thus the products of the tests and trials of the ages, must they not be in line with the realities of nature? Is it possible that a system of basally false thought, feeling and action has escaped disaster and has even guided evolution upward through hundreds of millions of years? The question is not

whether modes of thinking free from shortcomings, mistakes, illusions and even serious errors have been evolved, but whether basal soundness lies beneath them rather than fundamental falsity.

Now perceptions of space and time—as well as action based on such perceptions—have been matters of life and death to each of many generations of perceptive beings since the trilobites of the Cambrian seas gave chase or were chased and brought into service such perceptions of space and time as they then possessed. At least as early as this, eyes and other sense organs concerned in the perception of space and time had been developed to help in pursuit or in escape. These have been greatly sharpened in the course of subsequent ages. When a hawk plunges toward a coveted bird and the bird scuds away with his utmost speed, veering his course this way and that to escape, there come into play keen perceptions and quick uses of space and time as well as the relativities of pursuer and pursued. In such sharp contests it is quite essential to see whether the field is occupied with filled or unfilled space, for the latter alone is available for action. Out of the multitude of such critical actions grew the strong working sense of space so prevalent in the living world.

The sense of time appears to have grown up in close relation to that of space in so far as *action* was a part of the living experience, but when action ceased, time seemed to be independent of space, for time appeared to roll on while space remained unchanged. Thus a sense of the essential independence of time and space naturally grew out of working experience.

These mental reactions first appeared in the ancestry of man, but in the course of the ages they were continued onward and upward into the reactions of man himself, and became firmly fixed in his mental constitution as hereditary, organic or instinctive modes of mental action.

Along with these instinctive perceptions of the space that surrounded the evolving beings were the not less fundamental perceptions of extension in their own bodies. The hand, the foot and the mouth were easily perceived to be separate in space, and were also found to be separable in various degrees at will, while the time factor was small or nil and was subject to a different set of variations at will. Thus the will habitually adjusts time to space and space to time on the instinctive perception of their independence. All the senses played their parts in these practical combinations of space and time. Each sense tested the perceptions of the others. Their combined testimonies give unfaltering conviction of their essential soundness.

During the ascent of man, these deep-seated, organic, instinctive perceptions furnished material that was built into the natural sciences. Space relations on the face of the earth gave subject-matter for geography, topography, geology, geodesy, and the other earth sciences. Space measurements in the laboratories gave precise material for the mechanical, physical, and chemical sciences. The relations of space and time in the lower heavens entered into meteorology, while the vast relations of time and space in the outer heavens gave material for astronomy and astrophysics. All these sciences thus rest intimately on ideas of space as the

receptacle and natural frame of reference of all cosmic things, and on ideas of time as the tally sheet of successive events. The vital point here urged is that our ideas of space and time have deep organic rootage. They were not devised by Euclid, Galileo, or Newton. They were inherited by these master thinkers, whose contribution to us has been a clear and serviceable formulation of these ideas. It is not the personal views of these great men that are called in question so much as the instinctive organic reactions of the human race.

But strong as is this argument that a system of mental reaction evolved from the tests and trials of many millions of years is fundamentally sound, it does not reach, or even closely approach, inerrancy in details or even in vital matters; much less does it imply that the highest attainment has been reached. The very scheme of evolution implies indefinite struggle for closer adaptation of the active agents to the conditions under which they act. This leaves an open invitation for inquiry in all directions in the hope of reaching something that fits more closely the essential working realities. The argument does, however, carry the admonition that advocates of new adventures which strike at the roots of things should expect to find tremendous odds arrayed against them and should therefore put their views to long, severe and patient tests before assuming that they are true and before asking acceptance or beginning propagandism. If they undertake this they should give elaborate and explicit recognition of the old, and equally explicit expositions of the new.

True scientists have felt impelled all along to strive earnestly for the utmost precision wherever precision is important. As a result of persistent and scrupulous care, the existing sciences have been rewarded with great triumphs of precision. These serve as verifications of their fundamental trustworthiness. To the public, the prediction of eclipses stands forth as a most signal triumph and a verification of the Newtonian mechanics.

It is eminently proper, however, to challenge even these triumphs, but the challenge of the relativists has not lain against the trustworthiness of the Newtonian prediction of eclipses, nor against familiar triumphs of precision in other standard lines. It has been confined thus far to a few little known discrepancies of a very minute sort. And so Dr. Poor has found it obligatory to set forth with great care and precision the fundamental facts and principles necessary to a full and judicial opinion on the merits or lack of merit of the claims of the new views. It is an essential part of the purpose of this book to meet the challenge of the Einstein Theory of Relativity on its own selected grounds in so far as these are astronomical.

To fully appreciate the bearings of the Einstein view of relativity, it is well to recall the growth of the idea of relativity in the usual sense of the term, for relativity is nothing new. When a Cambrian trilobite chased some other -ite or was chased by it, there came into play relativities of space, time, strength, speed and skill. The great organic struggle of the ages from beginning to end was an intricate tangle of relativities. Moreover, these were constantly changing. As soon

as thinking reached a discriminating stage, it appeared that where there were relations there must be things to be related. And so, while relativities were taken into account, they were regarded as dependent on inherent qualities that were not merely relative. *The effort was to keep the balance between that which was dependent on relationships and that which would remain if the relationships were eliminated.* Even causal relationships are extremely numerous and highly changeable. For example, following the Newtonian doctrine that gravitation is universal, the motions of the earth are effected by the relative positions of all the other bodies in the heavens; its relativities of motion thus number hundreds of millions, and one set follows another every minute. It is absolutely impossible to deal with all or ascertain what is their sum total. Hundreds of millions of relativities must therefore be neglected to bring the case of the motions of the earth down to a workable basis. It has not seemed therefore that relativities as such were a promising line of attack. It has seemed better to deal with the most essential observed factors after the Newtonian method, part of which have a relative aspect and part, such as mass, inertia, and energy, an inherent aspect. A disproportionate stressing of relationships has been one of the sources of error all down the ages. The relations of the earth to the sun were the same as now in early historical times, but the ancients overstressed what was most obvious to them and thought the earth stationary while they made Phoebus drive his chariot across the sky daily, with the crystalline sphere following at night. Closer study showed that there were many relativities

of motion, vast spaces, great endowments of mass, inertia, energy, and other inherent properties. The recognition of these led to modern astronomy.

In a complex system like our cosmos motions must of course be relative. The relation is sometimes causal and sometimes merely incidental. In the standard modes of thinking, it has not been supposed that because a motion is relative it cannot also be inherent or absolute. For example, the earth and the sun revolve about their common center of gravity. The curvature of their paths is due to their mutual attractions—their relativity if you please—but if the mutual attraction were destroyed or neutralized, both sun and earth would move on in the line of their motions at the instant mutual gravity ceased to cause the curvature. This is due to inertia, an inherent, rather than a relative, property. *The destruction of the relativity of their motions would not destroy their absolute or inherent motions.* The relativity of their motions seems to be only a modified phase of the absolutivity or inherency of their motions. Right here lies the crux of the present issue. Einstein stresses relativity to the *exclusion* of the absolute or inherent. At least he says specifically that there is no absolute motion. It may be well at once to recognize that there may be misunderstanding of the term "absolute" and some other terms, for the usual senses of terms are not deferentially followed by the relativists and the non-Euclidian geometricians whose language they adopt. This group has reached preëminence in one field at least; they have achieved a nomenclature of the most distinguished infelicity attained thus far in the history of linguistic

endeavor. They speak of dimensions that other people merely call durations, of "fourth dimensional space," where other people feel able to think of only three dimensions, of relative motions that are devoid of absolute motion, and so on. In the standard way of thinking relativities of motion are phases of actual inherent or "absolute" motions. As the inherent are thought the more basal, they have been given precedence and the relativities fall into a secondary place. To make this vital point clear, let everything in the universe be wiped out of existence, except the possibilities of thought. Then let there be introduced a single natural unit, any unit that can have independent or inherent existence, say an electron, or an atom of hydrogen, or a quantum of energy. This unit cannot itself be a relation or a relativity, for a relation implies at least two things that are related. There can thus be no relativity at this stage. If there is any objection to this, will Einstein show us how a universe could be started with a relativity?

Let a second unit be added and relativity becomes possible. Let a third unit be added and the possible relativities rise to a number greater than the absolutes. If further absolutes are added, the number of possible relativities rises to even greater proportions. In a complex system, relativities are thus likely to become more numerous than absolutes. They are likely to become also more apparent because one sets off another. *But instead of the relativities putting the absolutes out of existence, these are required to make the relativities possible.* To the naturalistic thinker, *relativities thus serve as a form of proof of absolutivities.* The naturalistic

thinker does not see how relative motions can exist, if there are no inherently existent things to move. He does not see why a motion that is relative may not *also* be absolute. Unless the relative motions when combined algebraically reduce to zero it seems that there must be some absolute motion also. The thinking public would like to know if Einstein has proved that all motions would become nil if the existing relations of the moving bodies were blotted out. They would like to have the privilege of scrutinizing a putative *demonstration* that there is no absolute *motion*.

Further than this, it seems impossible for Einstein to deal with all the relativities of motion of the earth or of any other body; how then is such a demonstration possible? It is easy for Einstein to show the impossibility of dealing with the absolute in a full and final sense. Is it not equally impossible to deal with relativity in the same sense? Especially as there may be more relativities than absolutes?

Let us turn for a moment to some tenets that preceded the Einstein Theory of Relativity and led up to it.

First comes the gloomy forecast of Minkowski that "From henceforth [1908] space in itself and time in itself sink to mere shadows and only a kind of union of the two remains independent." The layman is puzzled to know just what this sinking of space and time into mere shadows means, as also just what the union product is, and why the union has independence when its constituents have none. Usually constituents are more independent and lasting than combinations. In the nomenclature of the doctrine of which this is a part, time is styled a dimension and is used mathe-

matically as a variable in common with space-dimensions. A "fourth dimension" and "fourth-dimensional space" carry the semblance of mystery into the literature of the doctrine. Perhaps it will help toward clarity to re-state Minkowski's forecast in more explicit terms: *From henceforth dimensions in themselves and duration in itself sink to mere shadows and only a kind of union of dimensions and duration remains independent.* This brings the forecast within the testing power of the public. To it life affords no more vivid, sharply defined concepts than those of the rooms and appointments of home, office, or shop. Equally vivid are the impressions of the public respecting great architectural creations. All these are combinations of dimensions and spaces. The very soul of architecture lies in adapting occupied space (walls, columns, etc.) to the enclosure of empty space of designed forms and dimensions suited to the special purposes in mind. Now has it seemed to designers in architectural planning, or to builders in construction, that there is any such inevitable union of dimensions with time as to render the dimensions shadowy if not combined with time? Do we all put a pinch of time into the dimensions of our living rooms and into the placing of furniture to forestall the shadows into which they otherwise would sink? If a consensus of the impressions of the best thinking people of the world today, in such matters—including expert designers and builders—were taken, would it not disclose the view that the senses of dimensions, irrespective of time, and of time, irrespective of dimensions, have each grown more distinct and precise as experience has

increased intelligence and capacity in these lines? It certainly puzzles the layman to form a clear-cut impression of how and why the concepts of space and time sink into obscurity and lose their independence if studied independently. It is equally puzzling to picture the precise nature of the union of time and space that is urged to take their individual places. If one looks to this postulated union for some inspiring esthetic effect to take the place of the old concepts foredoomed to sink into shadows, do any high lights stand forth? If a persistent student tries to follow the line of the new thought back to its source, is he likely to land elsewhere than in the infinitesimal, or the inaccessible, or the indeterminable?

To an investigator of experience, a feeling of insecurity in applying results arises, if time is put into the equations simply as a variable and is handled merely as a variable. The loss of any distinguishing mark in the computative process removes the result one step away from intimate dependence on the concrete, and one step toward that freedom in mathematical handling which comes of the absence of bondages to the actual. Every loss of such bondage to special fact unfetters the mathematical processes and gives them freer sweep and greater scope. From that point of view there is a gain, but it endangers the specific application of the results to the actual case. The thrifty workman multiplies five days' work by four dollars a day in sheer disregard of mathematical propriety; but he thereby holds fast to the facts of his toilsome lot. He would gladly multiply four days' work by five dollars a day, but his employer objects. Stripped of trouble-

some realities, five times four are twenty; the problem is easy and the process elegant. Three hundred thousand kilometers per second is mathematically much the same as 300,000 seconds per kilometer. If the labels are not religiously glued to the factors, the result is liable to become a composite product that cannot be readily unscrambled. Mathematics is undoubtedly the greatest achievement thus far attained by man through purely intellectual processes; it is to be held in high reverence on this account, but it is not to be forgotten that many of its great achievements have been attained by generalizations and abstractions that gave the mathematical processes the freest possible sweep. Great as are its virtues, it has not wholly escaped the vices of its virtues. Results reached by means of the abstract and the imaginary are likely to carry these qualities into the product. To secure concrete results the processes must be fettered by severe bondage to the facts, however trammeling these may be. And so the thrifty workman is true to the admonitions of experience in clinging fast to his method of multiplying days of work by the wage per day, for these are to him the hard realities.

FitzGerald and his followers, to meet the dilemma offered by the negative outcome of the famous Michelson-Morley experiments, advanced the view that the dimensions of all measuring rods vary not only according to the velocity of their motions but according as they move endwise or sidewise, while all timekeepers go slower as they are carried faster through space. Now our measuring-rods form the chief basis of precision in physics and mechanics. The accuracy

of time-keepers is equally indispensable in astronomical work. It is a further postulate of the new view that these variations cannot be corrected. Thus about all that is left to be trusted is a relativity which is itself subject to change and is presumably always changing. But when one recalls the almost infinite plexus of relativities into which one enters when he follows actions back into their interminable cosmic connections, he does not readily see how even such postulated variability can really be demonstrated. Are these postulates really in any sense demonstrations or are they merely weird speculations devised to escape the dilemma of a disappointing experiment?

A further step toward the Einstein theory of relativity was the electron theory of Lorentz. There is no question about the importance of the new revelations regarding the constitution of matter, nor any doubt that electro-magnetic dynamics play an important function in cosmic affairs, but all this is quite apart from the question whether these are to be interpreted as relativities or not. The electron is not typical neutral matter such as forms the basis of the Newtonian mechanics; it is merely one of the elements of such matter. The electron always appears to be derived from neutral matter or from matter negatively charged. Its strangest feature is that it appears to develop—or else to attach to itself—increased mass as its velocity is increased. This, taken by itself, may easily be thought to require a radical change in standard ideas of mass. But in the neutral state of matter, the electron is mated with an equivalent electrical charge of the opposite type. This opposite charge has never

been separated from matter and may merely be matter deficient in electric energy, as held by Franklin. If this is the correct view, the law that action and reaction are equal and in opposite directions seems to make it quite sure that when an electron is shot forth with its extraordinary endowment of energy, an equivalent *deficiency* of energy is left behind in the form of the opposite electric state. In a summation of effects on a large scale, as in astronomical problems, the balance between this extraordinary endowment of the electron and this equally extraordinary deficiency left behind, should give a result of the order of neutral matter, the basis of the Newtonian mechanics. As neutral matter overwhelmingly surpasses charged matter in the earth and as the increments and decrements of charged matter offset one another, the sum total of differences between electro-magnetic and ordinary mechanics largely disappears so far as astronomical problems and most ordinary problems are concerned. Before any material difference can be claimed, it is necessary to show that the earth as a whole is persistently charged either positively or negatively, and that the charge has a value large enough to be material when compared with the mass of the earth. Thus far, any such appreciable charge has seemed improbable; certainly it has not been shown to be important in astronomical calculations. But it is of course a matter that invites investigation, and may yet prove to have appreciable importance. But even then it will remain to be shown that it confirms the claims of Einstein Relativity.

The influence of electric and magnetic fields upon

bodies moving within them also needs recognition. When such influence is appreciable, the treatment must of course follow the laws of electro-magnetic dynamics rather than those of neutral matter. But here again it must be recognized that there are positive fields of force and negative fields of force and that, in any summation for a body like the earth, it is the algebraic sum of the two that represents the general effect. In the problems of great masses such as planets and stars, it does not appear from present evidence that electric and magnetic fields increase or diminish appreciably the value of the results given by Newtonian mechanics. In the study of very diffuse bodies, such as the comas and tails of comets, and the diffuse nebulas, the electric and magnetic states very likely may require that electro-magnetic mechanics supplement the results given by ordinary celestial mechanics. The interchanges of electrons between the various bodies of the heavens may have value enough also to require special treatment in addition to that of the Newtonian mechanics.

But until these several modifying influences of electricity and magnetism are shown to have value enough to require a modification of the Newtonian mechanics, the proper scientific attitude is that of reserve with an attitude hospitable to any result that may be supported by evidence as it accumulates.

All these phenomena stand on their own evidences and the question of relativity has about the same relationship to them that it has to the phenomena of neutral matter.

The question of the real merit of the Einstein contention then remains to be settled by careful and

critical tests. If the new candidate for acceptance does not choose to enter the field where the standard system has made its triumphs well known, but chooses its own tests in unfamiliar fields, the nature of these fields and the results of a strict application of the Newtonian and the Einstein systems respectively must be set forth with the utmost explicitness and fidelity to give the public a fair chance to pass upon the merits of the case. The purpose of this book is to do this for the two astronomical tests to which appeal has been made. The reader cannot fail to be impressed with the fullness, care, and explicitness of Dr. Poor's treatment.

THOMAS CHROWDER CHAMBERLIN.

THE UNIVERSITY OF CHICAGO,
August 30, 1922.

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CHAPTER I

THE THEORY OF RELATIVITY

THE THEORY OF RELATIVITY, as developed by Professor Albert Einstein, is an attempt to explain the apparent results of certain intricate optical experiments by a complete reconstruction of our fundamental ideas in regard to space and time. From the earliest days of scientific thought, time and space have been considered as independent: time flowing on uniformly regardless of the countless bodies in the universe and of their motions to and fro through endless and limitless space. An interval of time has always been regarded as the same under all conditions and throughout all space; an hour as identically the same for a person at rest and for an aviator flying at one hundred miles per hour, for a man in New York City, for an hypothetical inhabitant of Mars, and at the most distant star. But with Einstein all this is changed; space and time are bound together, space cannot exist without time, and time changes with space and with the motions of the

material bodies therein. According to the theory of relativity the interval of time, known as an hour, varies from place to place, it is different for the person at rest and for the aviator; it would appear longer for an astronomer on Mercury, and shorter to an observer on the slow moving Neptune.

Motion is the basis of our conception of space. We move freely about on the earth and, as we move, we encounter many objects; some freely moving and some apparently fixed. We soon learn, however, that rest and motion are relative terms; that objects, which from one point of view may be considered as fixed, are really in motion. To a passenger on a steamer the decks, the cabins, the port-holes are fixed; but the steamer is in motion and these relatively fixed objects are being carried from port to port.

Further the earth itself is in rapid motion; it rotates on its axis and it sweeps through space in a great curved path about the sun. And other bodies, similar to the earth, are found participating in like motions about the sun; Mars, Jupiter, Saturn, and hundreds of minor planetoids and comets. These, with the earth, form the Solar System, and their motions relative to the sun and to one another can be measured and their paths determined. But even the Solar System is not at rest; delicate and long continued investigations show that the sun and all its attendant planets are moving among the stars. The stars themselves are in motion.

All these moving objects, the earth, the planets, the stars must be in something, and that something, in which they exist and have their being, is what we call space.

Thus from our every day experience, confirmed by an endless series of measurements, is derived our fundamental concept of space—a general receptacle in which things have their existence. As all bodies, with which we come into contact, have three dimensions, length, breadth, and thickness, so the general receptacle must have three dimensions and must extend in all directions beyond the farthest known or imaginable thing. The general container, space, contains everything; it is itself, however, devoid of all material attributes; it is boundless, limitless. Each point of space is like each and every other point; each point has one attribute and one attribute only, that of position.

Material things move about, or are moved, in this general receptacle. Certain objects are noted as moving in cycles, appearing now in one position, now in another, and then back again to the first position; a flash of light appears, disappears, only to reappear again in the same place. To such recurrent phenomena is due the concept of time. Although each flash appears at the same point, yet each flash is different. Something has happened, and we need a system, a something to distinguish the one flash from the other. Time is such a system, it is the system by which we con-

nect together events as they actually happen; it is the wire, so to speak, on which we string the beads of successive events. In our concept we separate the system from the material bodies, and conceive of absolute, true time as flowing at a constant rate, unaffected by material things, or by the motions of material bodies. We conceive of time as being the same everywhere and under all conditions. The instant, known as 5 o'clock in the afternoon of June 10th in New York City, is a definite instant everywhere throughout space; the same in New York, in London, on Mars, on Jupiter, and on Sirius: a minute, an hour, a day, each measures identically the same interval of time at every point throughout space.

There is, however, an essential difference between space and time, or rather between our fundamental concepts of these. Space is reversible, time is not. One can freely pass from point to point in space and back again; within limits space is under our control, we can occupy a definite point or not at our pleasure. Not so with time, it flows uniformly in one direction, we pass forward but not backward, and the speed of the passage is beyond all control. We must pass forward in time, whether we choose or not, and all beings and all things pass forward, in time, at the same rate. One can travel from New York to Boston and back again at pleasure; but one cannot pass from to-day to yesterday, nor from yesterday to to-morrow: the days pass and time rolls

on without check or hinderance, and is the same for every being and for every material body in the universe.

Space is three dimensional and reversible; time is of one dimension and irreversible. Both time and space exist independently, and independent of any material thing, or body.

There is a third concept, not so fundamental as those of time and space, but still essential to our ideas—the concept of the ether. The necessity for the ether arises from modern experiments with light. Light is emitted by a candle, by the sun, by a distant star, and some how, in some way, is transmitted through space to the eye of an observer. Sir Isaac Newton thought of this transmission as the actual passage through space of minute particles of matter; he thought a luminous body shot forth, in all directions and at immense speed, continuous streams of extremely minute particles, or corpuscles. To him space could be an empty void, in which moved the material bodies of the universe and through which were shot the minute corpuscles of light. This theory explained all the phenomena known at the time of Newton, but it cannot explain phenomena known to-day, nor experiments which can be made with simple apparatus. It is now known that no actual transmission of matter takes place when light passes from a candle to one's eye; light is now known to be a mode of motion, a type of wave motion analogous, in a remote degree, to the waves of the ocean. These

water waves travel along the surface of the ocean, but the water itself remains practically at rest; each little particle of water rises and falls with the wave, and then, after the wave has passed, it returns to its original position. The waves produced by a hurricane in the southern seas travel for thousands of miles to break at last as a gentle surf upon the beaches of our northern coasts, but the warm waters of the southern seas remain in the south, and the surf on the beach is formed of the icy waters of the north. Just as there cannot be ocean waves without water, so we cannot conceive of light waves and the propagation of light from sun to earth and from star to star without an interstellar ocean or medium. And this medium, which we cannot see, nor feel, nor weigh, is the so-called ether of the physicist. It is the ocean which fills all space and in which float the earth, the sun, the planets, and the unnumbered stars, and throughout which are passing, in every direction, the waves of energy, which we know as light and radiant heat.

In this conception of an all-pervading medium the physicist has met many difficulties: certain phenomena apparently necessitate its having definite properties as to elasticity and rigidity, other phenomena apparently require different and antagonistic properties. The need of such a medium is essential to clear thinking, but experiments have failed to determine its specific qualities. One property, one attribute, however, has been

considered as fully determined—the ether has been thought to be at rest, to be as a whole forever stationary. The earth, the planets, the stars, all move through this ethereal ocean without disturbance, without friction.

If the ether be at rest, then light and light waves should furnish the physicist with a very delicate way of measuring the direction and speed of the earth through this medium; the ether should furnish an absolute standard to which can be referred all the varied motions of the planets, and from which their exact positions and motions in and through space can be determined. Many experiments, therefore, have been made to detect and to measure the motion of the earth through the ether, or the "ether-drift," as it is called, but without definite results. The most famous attempt was the Michelson-Morley experiment made in 1887. As this experiment is the basis of the Relativity Theory, the principles involved in it should be thoroughly understood. Einstein, himself, refers to it as "decisive."

Light, as has been seen, is a wave motion in the ether; a disturbance that, once set up, travels through the ether in all directions at a terrific speed. The earth also moves through the ether, but at a very sedate pace as compared to light waves. If the earth and a ray of light be travelling in the same direction, then the ray will overtake the earth and pass it, but it should

pass more slowly than if the earth were at rest. Again, if the ray and the earth be travelling in opposite directions, the two should pass at a much greater relative speed. It is the case, if you like, of two automobiles on a country road: if they are both travelling in the same direction, the faster car gradually overhauls and passes the slower; while, if they are travelling in opposite directions, they flash by in a moment. Now, the speed of light is so enormous as compared to that of the earth, one hundred and eighty-six thousand (186,000) miles per second as against only nineteen (19) miles, that it is impossible to measure directly the relative speeds of a ray, when passing and when overtaking the earth. When passing, the relative speed would be 186,019 miles, when overtaking, 185,981 miles: no instrument, no method yet devised, is delicate enough to measure the speed of light to within the 38 miles difference between these quantities.

What cannot be done directly, however, may be done indirectly. Thus Michelson devised a method and instrument for measuring, not the actual speeds of the two rays of light, but the ratio of their speeds. By an ingenious arrangement of mirrors he split up a ray of light into two parts and sent these two rays, or parts of a ray, over two different paths of the same length, the two paths lying in different directions relative to the motion of the earth. Upon the completion of their respective journeys, the two rays re-

turned to the starting point, where the minute interval between the times of their respective arrivals was accurately measured. From this measured difference in the times required for the rays to travel the same distance, the difference in their respective speeds can, of course, be found by simple calculation.

A simple illustration will make the underlying principle of this experiment clear: an illustration that has been used many times. But first, it will simplify matters perhaps, if we consider the earth at rest and the ether as drifting by. The motion of the two—the earth and the ether—is relative, and it makes no difference to which we attribute the motion. Consider a motorboat, capable of running 10 miles per hour in still water, making regular trips up and down, or across, a river in which there is a steady current of two miles per hour. On a trip up the river against the current the boat will make a net gain, by the shore, of 8 miles per hour; running down the stream the boat will pass the shore at the rate of 12 miles per hour. It would take the boat 30 minutes to go up the stream 4 miles, but only 20 minutes for the return trip with the current; a total of 50 minutes for the whole return trip of 8 miles. In still water, however, the boat, travelling at the rate of a mile every six minutes, could make an 8 mile trip in 48 minutes. In other words, although the boat travels the same distance with and against a river current, yet the effect of the current is to retard the

boat, to lengthen the time required to make a complete round trip.

A moment's consideration will show that a similar, though smaller, retardation takes place when the trip

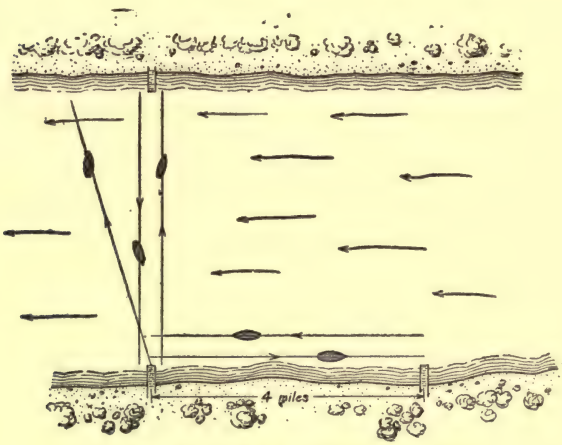


Fig. 1. Retardation Effects of a Current.

is made across the current in any direction. If the boat be headed directly across the stream, it will, at the end of 24 minutes, have travelled 4 miles, but, during this time, the current will have carried it down stream nearly one mile. In order to reach its destination the boat will have to be headed up stream against the current, and this extra mile or so up stream will take several additional minutes. Such a course unduly lengthens the trip; the quickest trip across the river can be made by heading the boat slightly up river

against the current, just enough to counteract the drift, and by keeping the boat on this one straight course throughout the trip. To make good on such a course the boat will have to be headed up river about a compass point and the trip across the river will require 24 minutes and some 29 seconds. The return trip will be made in the same time, so that the entire 8 miles across the river and return will require 48 minutes and some 59 seconds: or the retardation will amount to only some 59 seconds, as against 2 minutes for the trip up and down stream.

Thus in any return trip the traveller, be it a motor-boat or a light wave, will suffer a retardation in time, if the trip be made in a moving medium. This retardation will be the greatest when the trip is directly with and against the current, smallest when directly across the current. Again the retardation becomes smaller and smaller as the speed of the traveller increases in proportion to the drift of the medium. In the case of a light wave, the speed of the wave is very great in comparison with the speed of the earth through the ether, and the retardation is, therefore, extremely minute. In the Michelson-Morley experiment the return trip was but a few yards long, and the calculated retardation amounted to only a very small fraction of a *billionth of a second.*

To measure such an apparently hopelessly small interval of time Michelson devised a most ingenious in-

strument, in which he utilized the time of vibration of a light wave as his standard measure of time. Light waves vibrate, or follow one another, at the rate of about six hundred thousand billion a second; and it was this interval of time that Michelson used to measure the relative retardations of the waves travelling in the two directions. His instrument, therefore, was capable of measuring relative retardations many times smaller than the calculated value. The optical

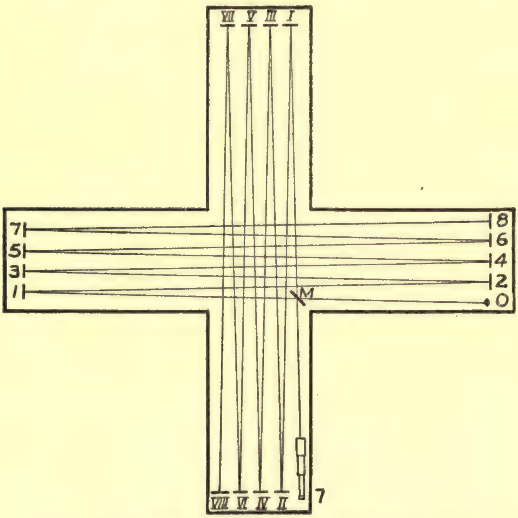


Fig. 2. The Michelson-Morley Apparatus

principles upon which this measuring device is based are those known under the general head of "interference." An explanation of these principles and of the details of the device itself would be out of place here;

such explanations can be found in regular text-books. It is only necessary to state that the device is founded on sound principles and is capable of detecting the minute differences required.

It is different however, with that part of the whole apparatus which contains the paths of the rays through the ether. This must be clearly and fully explained, for upon the results obtained by the use of this apparatus depends the whole theory of Relativity.

The apparatus, made of steel, was in the form of a cross which floated in a trough of mercury. At the end of each arm four mirrors were placed and so arranged as to reflect a ray of light back and forth between them. A ray, starting at T in the diagram, reached a mirror at M, where it divided into two parts. The first part travelled straight on to the mirror 1, where it was reflected back to a corresponding mirror at the other end of the arm; thence it was reflected back and forth from end to end of the arm until it reached the last mirror. Here it was returned over its course from mirror to mirror back to M again, where it was reflected to the observer, with the measuring device, at O. The second part of the ray was reflected at M into the transverse arm of the cross, where it was reflected back and forth from mirror to mirror until it also reached the observer at O. The two rays, or rather the two parts of the same ray, thus travelled over different paths at right angles to each other and the re-

tardation of the one ray over the other was measured by the observer with his delicate interferometer.

Of course is is absolutely impossible to set up such an instrument, involving the placing and adjustment of many mirrors, so that the two paths are exactly equal in length and so that each and every mirror is in its exact alinement. Nor can we know at any instant the exact direction in which the earth is plowing through the ether, so that it is impossible to place one arm of the instrument parallel to the ether-drift and the other at right angles to it. In any one fixed position of the apparatus, therefore, an observed retardation of one ray over the other might be the indication merely of instrumental errors of adjustment, errors in the lengths of the arms, in the alinement of the mirrors, or in the direction of the instrument as a whole. But if the apparatus be rotated so that the arms take up various positions with respect to the drift, then the retardations due to instrumental errors will be eliminated, and that due to the drift will show up.

Such observations were carried on over a long period of time, at different hours of the day, at different seasons of the year, in different places and under different conditions. The results were *negative;* no constant measurable retardation in any particular direction was observed. To interpret this result, return for a moment to our motor-boat illustration. If such a boat made four mile return trips, up and down stream,

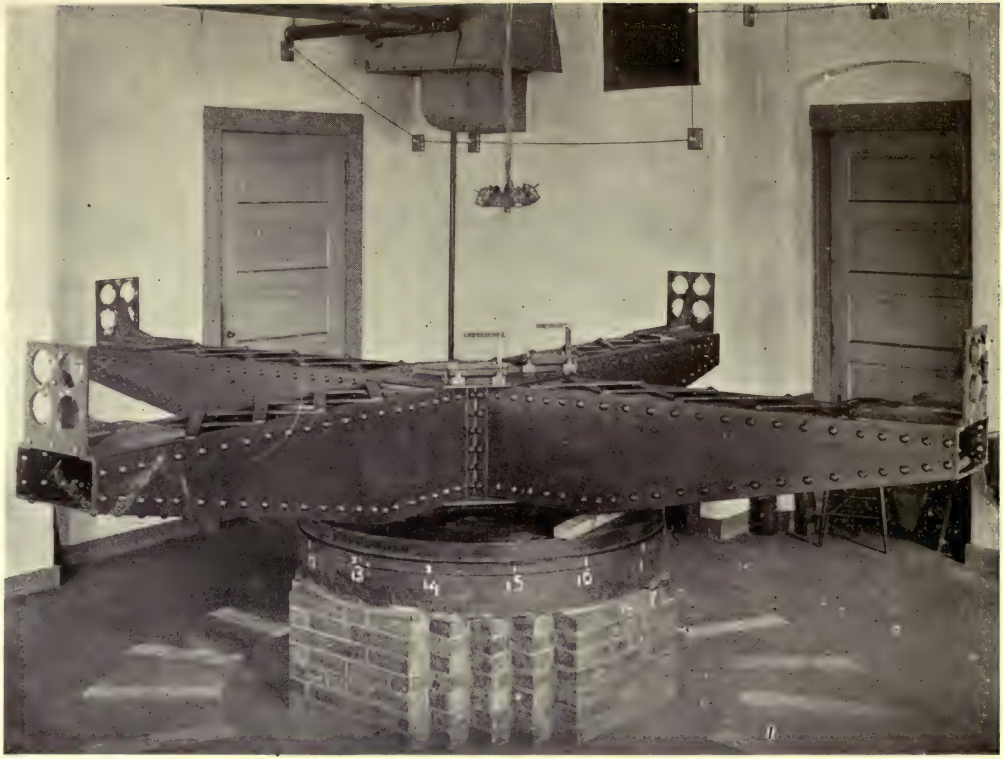


Plate 1.

Ether-Drift Interferometer, as used by Morley and Miller in 1903–1905.

The experiments made with this instrument, in Cleveland, gave negative results: no ether-drift was found. Einstein calls these experiments decisive and bases his entire theory upon the failure to measure an ether-drift by Michelson and Morley in 1887, and by Morley and Miller in 1904.

across the stream, and in any and every direction and every trip was made in exactly 48 minutes, then one and only one conclusion could be drawn:—there is no current, the water in the river is at absolute rest. Similarly, on the face of the Michelson-Morley results, light passes an observer on the earth, at all times and in all directions, at the same speed; the earth and the ether, if indeed there be an ether, move together.

The results of the Michelson-Morley experiments, if accepted as conclusive, affect the physicist's conception of a motionless ether filling all space, but they do not necessarily affect in any way the fundamental concepts of time and space.

After many fruitless attempts on the part of physicists to find a satisfactory explanation for the remarkable and unexpected results of this experiment, Lorentz suggested the contraction theory, known under the name of the Lorentz-FitzGerald theory. Under this theory all bodies in motion are compressed, or shortened, in the direction of the motion. The amount of this compression depends upon the speed; as the speed increases the contraction becomes greater and greater. A steel rod, for example, may measure exactly one yard while at rest, but, when set in motion in the direction of its length it actually becomes shorter. A mere shift in direction may change the length of the rod, for the rotation of the earth carries all bodies on its surface in an eastward direction at a relatively

high speed, and a rod, therefore, under this theory will actually be shorter when it points east and west than when north and south.

Such changes in length, if real, cannot be directly measured, for all bodies contract when placed in motion and, therefore, a yard-stick will change in exactly the same way and in exactly the same proportion as the body to be measured. But indirectly through experiments with light, as in the Michelson-Morley experiments, the effect of the contraction becomes apparent and measurable. Returning to our illustration of the motor-boat and its trips in the river, it will be recalled that it required about one minute more for a return trip with and against the current, than for a return trip of the same length across the current. But, if now the up and down trip be shortened by a small fraction of a mile, leaving the across current trip the original length, then the two trips might be made in identically the same interval of time. So with the Michelson-Morley experiment, if the length of the arm of the cross automatically becomes shorter when parallel to the motion of the earth through the ether, then the time interval for light rays travelling in this direction will be reduced; and, if the shortening be in exactly the right proportion, then the two intervals for the rays along the two arms will be the same, and the negative result of the experiment fully accounted for.

The actual amount of contraction required to explain the experiments in this manner is very minute, and depends, of course, upon the proportionate speeds of light and of the earth through the ether. At the speed with which the earth is moving in its orbit, the necessary contraction in the length of a body is less than one part in one hundred million: about one inch in the distance between New York and Denver.

This contraction theory of Lorentz-FitzGerald is a possible explanation of the experiment; it is one of several possible explanations. Einstein offers another solution of the problem; a solution of a radically different sort, one which involves the fundamental concepts of time and space.

Einstein, as the very basis of his theories, assumes that the Michelson-Morley experiment is *"decisive"*; that *"there can be no ether-drift, nor any experiment with which to demonstrate it"* (63).* He contends in substance that we have exhausted all means of research in our attempts to measure absolute motion through space, and that, having failed, it is because the universe is so constructed as to make it impossible

* *Relativity, the Special and General Theory,* by Albert Einstein. Translated by R. W. Lawson, 1920, page 63.

The followers of Einstein do not always agree among themselves as to the details of the theory and as to the meaning of certain principles and statements. I have, therefore, in my attempt to explain the concepts and theories of Einstein, confined myself to a study of his own work. The many references to this work are noted in the text by the number of the page on which the quotation, or statement, is to be found.

ever to detect by any physical experiment, optical or otherwise, the existence of absolute motion, or motion through any ether, or other medium, which may pervade all space. His course of reasoning appears to be, that, as it has been "decisively" shown that we cannot measure absolute motion, therefore absolute motion does not exist. This is the same course of reasoning that led Congress to refuse money to Langley for a continuation of his experiments with flying machines: —all experiments had failed, Langley's machine had fallen into the Potomac, and, therefore, a successful flying-machine was an utter impossibility. But, congressional reasoning to the contrary notwithstanding, we now have flying-machines; machines built upon the very principles upon which Langley was working.

Einstein asserts that the Michelson-Morley experiment is final and conclusive, and he explains the result of that experiment by the assertion that there is no "absolute motion," there is no "absolute space," there is no "absolute time." All motion is relative: the steamer moves relatively to the earth, the earth moves relatively to the sun, the sun relatively to the stars. Nothing exists independently of the observer; all is relative, nothing is absolute. Hence the name:— RELATIVITY THEORY.

This general postulate, or assumption, of the relativity of all motion and the non-existence of absolute motion is explained, illustrated and enforced by Ein-

stein in the following way. He supposes two observers, one in a railroad train running on a straight stretch of track at uniform speed, the other observer standing beside the track and watching the train go by. Just as the train passes the watcher on the ground, the person in the train leans out of the window and drops a stone. This stone partakes of the forward motion of the train, and to the person who let it fall it appears to fall in a straight line, as shown in the accompanying diagram. But to the watcher on the ground, the stone, as it falls towards the earth, appears to move forward in the same direction as the train; to him it appears to describe a curved line, a parabola.

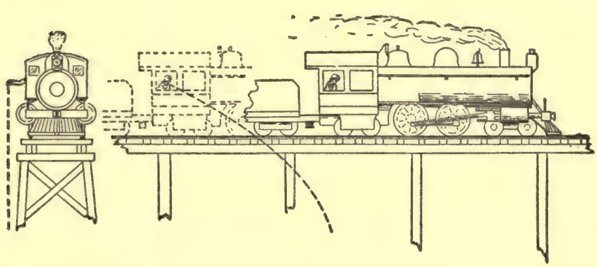


Fig. 3. The Relativity of Motion.

Which, if either, is the true path of the stone; the straight line as it appears to the one observer, the parabola as the other sees it, or is it some other curve, compounded of these and the motion of the earth? To Einstein the answer is simplicity itself, the stone

has no path. *"With the aid of this example it is clearly seen that there is no such thing as an independently existing trajectory (lit. "path-curve"), but only a trajectory relative to a particular body of reference"* (10).

What does this statement of Einstein mean? The stone certainly left the window of the car and came to rest at some point on the ground at the side of the track. How did it get from the window to the ground? The ordinary common-sense answer would be that the stone travelled in some curved path through space, from one point to the other. It is true that this path might appear differently to different observers; as a straight line to a person in the train, as a parabola to a watcher on the ground, as a twisted curve to an aviator flying diagonally over the train: but, no matter how the path appeared to these various observers, the stone travelled in one single definite path; it would have travelled the same path, if no one had watched it fall. Now this simple common-sense statement, that the stone actually did pass from the one point to the other in some definite path, is exactly what Einstein, in his statement above quoted, denies. He states that the stone had no path independent of an observer, that the path it travelled depended upon the person who watched it fall, that it actually had different paths for the different observers. This is the essence of relativity: the path that the stone travels is a joint phenome-

non of the observer and of the stone. In the absence of either the observer or the stone there would be no path: the path, a joint phenomenon of the observer and the stone, exists only in their joint presence.

This postulate of relativity is the denial of the existence of any reality behind our observations. The physical, material world of land and water, of trees and houses, of men and women does not actually exist; it is not a real world. It exists only in and through the observer, and is different for different observers. The path of a falling stone is a straight line for one observer, a parabola to another; a steel rod is a yard long to one person, and a different length to another; each and every observer is correct; the stone has no "true" path, the rod has no "real" length. The fantastic picture of the cubist is as true to nature as the work of a Corot or a Meissonnier.

This postulate of relativity can be expressed also in mathematical language; in terms of systems of coördinates, frames of reference, and of transformation equations. While these words may be unfamiliar to the reader, the things themselves are in constant, daily use. The charts showing the fluctuations of the stock market, the engineer's diagrams showing the relation between speed and engine power, are both examples of the use of a system of coördinates. In the first case the horizontal lengths in the diagram are made proportional to the time, to weeks, months, or years, and

the vertical lengths proportional to the price of stocks. Such a diagram gives at once the price of the stock on any date, and the curve connecting the various prices shows clearly the fluctuations in the market price. The two heavy, fixed boundary lines of such a diagram, one horizontal, the other vertical, are technically the axes, and the lengths, which locate any point on the price curve, are the coördinates of that point. Any number of diagrams may be made to represent the same price fluctuations: the unit of time may be the week, the month, or the year; the unit of price may be the dollar, the pound, or the franc. In changing our diagram from one price unit to another, from the dollar to the franc, or from the franc to the pound, we must have a definite relation between the units, we must know the "rate of exchange." This rate of exchange may remain sensibly the same for long periods in normal times, or it may suffer violent changes from day to day, as in the abnormal times of war.

In a manner entirely similar, the mathematician specifies the position of a point in space by referring it to three mutually perpendicular planes and calls such a system, a system of coördinates and coördinate planes. If to such a system a clock be added, so as to fix the time of an event as well as the position in space at which it occurs, we then have a Frame of Reference; and each observer of physical phenomena is supposed to have such a frame, to which he is rigidly attached

and with which he moves. To return to the example of the moving train, the observer who drops the stone refers its motion to a frame rigidly attached to the train and partaking of the train's motion; the watcher on the ground, on the other hand, refers the motion of the stone to his frame of reference which is rigidly attached to the earth. The diagrams, or equations, which the two observers draw or compute to represent the motions of the falling stone will be different, but from one diagram we ought to be able to construct the other, provided we know the relative motion of the two frames, provided, in other words, that we know the rate of exchange. In technical language, to pass from one reference frame to another we need "transformation equations."

Now the observer in the railroad carriage will formulate laws of motion and of falling bodies with reference to his frame; the watcher on the ground will similarly formulate laws in reference to his frame. If the laws, thus formulated by the two observers, are fundamental laws of nature, they should be identically the same; the two systems should be equivalent for the description of natural phenomena. When the observed phenomena are transferred from one frame of reference to the other by the use of the proper transformation equations, then the fundamental laws derived from the observations should be the same and should be expressed in identically the same form. According

to the relativitists, all laws of nature can or should be enunciated in such forms that they are as true, in these forms, for one observer as for another, even though the observers, with their frames of reference, be in uniform motion, without rotation, relative to one another. According to Einsein, when K and K′ represent two different coördinate systems, this idea is expressed in the following explicit terms: *"If, relative to K, K′ is a uniformly moving co-ordinate system devoid of rotation, then natural phenomena run their course with respect to K′ according to exactly the same general laws as with respect to K. This statement is called the* principle of relativity *(in the restricted sense)."* (15).

Now the principle of relativity stated in this mathematical form is equivalent to, or tacitly involves, the denial of "absolute rest": it means that there is, in the universe, no body, no thing, completely at rest; that there is no motionless ether. For, with respect to a system absolutely at rest, with respect to a motionless ether, the natural laws of motion are capable of being formulated, or expressed, in a particularly simple and unique manner: such a system, therefore, is not equivalent to other systems. Thus the mathematical statement of the relativity principle means the same thing and involves the same ideas as the statement of the principle, heretofore made, in general, untechnical language.

THE SECOND POSTULATE, or principle, announced by Einstein, states that the velocity of light in free space *appears* the same to all observers, regardless of the motions of the source of light and of the observer. This is quite different from the old idea, or assumption, that the velocity of light in space is universally constant. The constant, c, for the velocity of light in the Einstein formulas is not the actual velocity of the wave in space, in the ether, but refers to the observer's measured value of this velocity with respect to himself.

In order to understand just what this postulate means, consider again the case of the motor-boat in the river. The current is running down stream, from left to right, with a speed of two miles per hour, and the boat is capable of running ten miles per hour in still water. If a floating log and the motor-boat start side by side in the stream opposite a point, A, on the

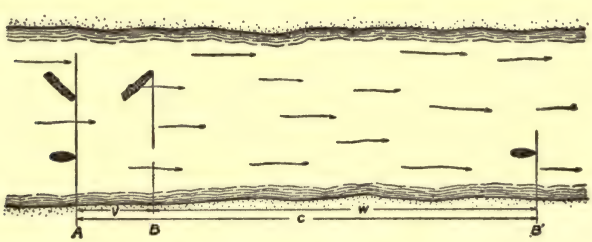


Fig. 4. Addition of Velocities.

river bank; then at the end of an hour, the log will have floated with the current and will be opposite a

point, B, two miles below A, but the motor-boat, running at full speed, will have been carried two miles by the current and ten miles by its own power, or will have reached a point, B′, twelve miles down stream. In other words, the boat will have passed down the river by a distance equal to the sum of the distances travelled by the current and by the boat itself. This is the ordinary theorem of the addition of velocities. It is in constant daily use, in one way or another, by practically everyone; it is taken so much as a matter of course that no one for a moment doubts its truth and validity.

This theorem can be expressed numerically in the form of an equation. Let us put v as the speed of the current, and w as that of the motor-boat in still water. Then the total actual speed of the boat down the river past the shore, or the distance covered in one hour, will be

$$c = v + w$$

In the example, v was equal to 2 miles, w equal to 10 miles, and c, the actual distance down stream in one hour, was, as has been seen, 12 miles. If the boat ran up stream against the current, then v would be subtracted from w, and the boat would pass the shore at the rate of 8 miles per hour. Now, by giving v and w appropriate values, this equation can be made to apply to any case that may occur, to streams of various

velocities, to motor-boats of widely different speeds, to railroad trains and to men walking, to airplanes and to winds of various strengths. But always, under all conditions, for all velocities, for boats, for men, for airplanes, c is invariably equal to the sum of v and w.

Now Einstein applies this ordinary theorem of our every-day life to the question of the speed of light as determined by two observers, one in a railroad train running at high speed, and the other one on the ground. In this application, the river becomes the train and the motor-boat becomes the wave of light. The speed of the train is v, and the speed of light relative to the train is w, and, therefore, our equation gives us for the speed of light relative to the ground,

$$c = v + w$$

or, the velocity of light, as measured by the observer on the ground, should be greater than that measured by the observer in the train; it should in fact be exactly equal to the sum of the speeds of the train and of light, as measured by the observer in the train. To put this another way, the wave of light should pass the observer at rest on the earth faster than it could overtake and pass the train, just as an automobile passes a man by the roadside more quickly than it can catch up to and pass a car running in the same direction.

This apparently simple and common-sense result is,

however, according to Einstein, not correct. He asserts that, in the case of light, c is always equal to w. Applied to a motor-boat and a running stream, this means that the boat would appear to go up stream against the current, or across the current, or down stream with the current at exactly the same speed: applied to an automobile, that it appears to pass the man by the roadside and the fast moving car at exactly the same rate. Einstein asserts that the velocity of light appears the same to every observer; for, according to the principle of relativity, the law of transmission of light *must* be the same for the railway carriage as for the ground. Further, the "decisive" experiment of Michelson and Morley proved this to be the case; the velocity of light, as measured by them, was always the same, no matter what the motion of their apparatus.

Of course, it is self evident that so long as the quantities in our equation have their common, everyday significance, c and w cannot be equal, unless v is zero. But Einstein states that c and w are always equal, no matter what values v may have. Therefore, if the theory of relativity be true, these quantities, c, v, and w, must differ in some way from our ordinary conception of them. They are all velocities; ratios of a distance divided by a definite interval of time; feet per minute, or miles per hour, for example. They, therefore, involve measures of both distances and times.

And heretofore, when measuring distances and times, we have, in our common-sense way of looking at things, assumed that:

1. The distance between two points of a rigid body is independent of the condition of motion of the body of reference: that is, a yard-stick is a yard long whether on the ground or in the train.
2. The time interval between two events is independent of the condition of motion of the body of reference: that is, a second of time for an observer on the ground is exactly the same interval as a second of time for the observer in the train.

These assumptions, which seem perfectly natural and in accord with our every-day experience, are, according to Einstein, "*unjustifiable*" (36), and, if discarded, then the theorem of the addition of velocities becomes invalid, c and w may always be equal, and the Theory of Relativity emerges triumphant.

This statement of Einstein means one thing and one thing only: that a rigid body appears to be of different lengths, when in motion and when at rest; that an interval of time appears to be different to an observer at rest from what it does to an observer in motion. The mathematical part of the problem is to find some definite relation between lengths and intervals of time, as measured by observers at rest and in

motion, such that the measured velocity of the ray of light shall always be the same, such that c shall always be equal to w in our equation. This condition of equality, essential to the relativity theory, leads to a perfectly definite relation, to specific transformation equations or, in banking parlance, to a fixed "rate of exchange." These equations are known as the "Lorentz Transformation Equations," and they involve the velocity of the moving body and the velocity of light.

In accordance with these equations, the length, a, of a rigid body in motion with a velocity v is altered so as to become:

$$a \times \sqrt{1 - \frac{v^2}{c^2}}$$

where c is the velocity of light. That is, according to Einstein, *"The rigid rod is thus shorter when in motion than when at rest, and the more quickly it is moving, the shorter is the rod"* (42). In simple language, this statement of Einstein means that a yard-stick is shorter when it is placed lengthwise on the floor of a car travelling forty miles per hour, than when at rest on the ground; that it becomes still shorter when carried by an airplane at one hundred and fifty miles an hour. It means that, if such a yard-stick be pivoted on the wing of an airplane in flight, it automatically increases and decreases in length, as it is placed parallel to the direction of flight, or cross-wise

on the wing. Further, from this equation it would appear that, if a material body could be given the velocity of light, its length would become zero; for in this case the fraction v/c would become c/c, or unity, and the quantity under the square root sign would thus become zero. The velocity of light, c, is thus a limiting velocity, which can never be exceeded.

Now although this assertion, or assumption, of Einstein may, at first, appear strange and contrary to common-sense, yet it does not directly conflict with our old fundamental concepts of time and space. Bodies contract under pressure, they expand with heat, and such expansion and contraction is perfectly understandable and in accord with the concept of space. It does not require any very radical change in our fundamental concepts, or methods of thinking, to conceive of motion as acting upon a body as a sort of pressure and of making it actually smaller. This is the Lorentz-FitzGerald contraction theory, as heretofore explained.

Further, as to time and intervals of time, the Lorentz transformation equations show that a time interval, t, changes for a body in motion and becomes:

$$\frac{t}{\sqrt{1 - \frac{v^2}{c^2}}}$$

The larger the velocity, v, of the body, the smaller the denominator of this fraction and the greater the

time interval. The time interval being larger, a clock must run more slowly, or as Einstein puts it: *"As a consequence of its motion the clock goes more slowly than when at rest"* (44).

Now this statement, or assertion, or assumption of Einstein in regard to time and time intervals is in direct opposition to our fundamental concepts, it violates our whole mode and method of thinking. Heretofore, time has been thought of as being independent of everyone and everything: time was the same for all portions of space, for all bodies, whether in motion or at rest: a minute was a minute the world around and everywhere in space. This identity of time and time intervals Einstein denies: according to his relativity theory, time depends upon motion, and every body has its own particular time: *"unless we are told the reference-body to which the statement of time refers, there is no meaning in a statement of the time of an event"* (32). The faster the body moves, the longer become the time intervals. The earth travels about the sun at a rate of 19 miles per second, Mercury at from 23 to 35 miles per second depending upon its place in its orbit, and Neptune at only 3⅓ miles per second. To an observer on Neptune, therefore, the interval of time, which we know as a year, would appear shorter, while to an astronomer on Mercury the year would appear longer. While the speeds of the planets thus differ greatly, yet they are all very small

fractions of the speed of light, and hence the variations in time intervals will be very minute, only about one part in a hundred million. The lengths of the year, as measured on the earth and on Mercury, would differ by only a couple of seconds or so.

This basic idea of the relativity of time should be thoroughly understood, and a further illustration may aid one in forming a conception as to what relativity, as enunciated by Einstein, really means. A clock goes more slowly when in motion than when at rest; the faster a body moves the longer the time intervals. As the speed of a body increases, therefore, a clock runs more and more slowly, and each minute and second becomes longer and longer. Supposing that as I write these words, my room could be sealed up and shot off into space with a speed approaching that of waves of light, one hundred and fifty, or one hundred and seventy thousand miles per second. As the speed of my room increased, my desk clock would run more and more slowly, each tick would represent the passing of an hour or a day, perhaps even of a year of ordinary earthly time. I would not know the difference, my heart would beat regularly, but each beat would mark the passing of months. At the end of half an hour by my clock, and before this paragraph could be completed, my room would have traversed the depths of space and been returned again to my island home. But, as I glance up from my paper,

what a change of scene! The peaceful bay on which my windows give, would be filled with strange craft, alien peoples would troop around, and I would learn that America had decayed and fallen, as Rome fell, centuries before. The names of Harding, Lloyd George, Poincaré would be meaningless; the World War even would have been forgotten, or remembered only to plague some schoolboy with the histories of past and long forgotten races. This is what relativity of time really means.

Now motion takes place in space and, if relativity be true and time varies with motion, time and space can no longer be considered as independent: they are bound together in some way, neither can exist without the other. A point in space cannot be thought of without predicating a time, and an interval of time has no meaning except in connection with a definite moving body in space. It is perfectly true, if such a phrase be allowable in a discussion of relativity, that, even under the old concepts of time and space, it is extremely difficult to conceive of time without space, or of space without time; yet the essential point of the old concepts, the independence of space and time, is easy to understand: that an instant of time is the same instant throughout all space, that an interval of time is the same, whether measured in one part of space or in another, upon a body at rest or upon a body in motion. It is this essential point in the old theories

that the relativitist denies. Time, according to the new theory, is relative, relative to position in space and to the motions of bodies therein. An interval between two events is not a fixed, definite interval; it is longer or shorter depending upon the speed with which the observer is moving through space.

If space and time are thus connected, it should be possible to express the connection in terms of mathematical symbols and equations. It is at this point and for this purpose that the formulas and methods of Four Dimensional Geometry are introduced into the relativity theory. To most people, the very words, four dimensions, are enough; everything at once becomes incomprehensible and absurd. Yet there is no reason for this too prevalent idea: in the broad sense of the words, there is nothing new or startling in the four dimensional idea. It is a matter of common, every-day knowledge that, in order to describe fully an event, we must tell not only where the event took place, but when. To speak of the Battle of the Marne does not definitly fix the event, for more than one battle was there fought; in the World War there were at least two distinct battles on the Marne. The when, the date, is essential if we are to particularize a certain definite battle, or event. To fix definitely the place at which an event occurs requires three elements, or coördinates, for all objects in space have three dimensions, length, breadth, and thickness. To locate the

place of the battle the three place elements, or coördinates are, the surface of the earth, the latitude, and the longitude of the point on the river at which the battle was fought. But to identify the First Battle, or the Second Battle, or any particular event of either we must have a fourth element, the date on which the event happened. That is, to completely identify any event, a battle on the earth's surface, the fall of a meteor, the collision of two stars, we must have four elements; three to fix the position in space, and one to fix it in time. Thus, in the broad, general sense of the words, we live and have our being in a world of four dimensions, and mathematically speaking it requires four numbers, statements, or coördinates, to identify fully, in space and time, an event or happening.

There is nothing new in all this. But what is new, what is startling, is the point emphasized in the relativity theory, the introduction of a definite mathematical relation between the space coördinates and the time coördinate. This relationship is introduced through the adoption of the Lorentz transformation equations, heretofore fully explained, and was specifically brought out by Minkowski. It can best be understood by means of an illustration.

In our ordinary, every-day world two surveyors compare their results as to the positions of two definite points, two boundary stones, or markers, for example. One surveyor runs his lines as his compass points,

north and south, east and west; the other, knowing that the compass does not point towards the true north, corrects his compass courses and runs his lines parallel to and at right angles to the true meridian. The actual measures of the two surveyors will differ, their lines, as run, will be of different lengths; but, if their work be accurate, their final results as to the actual distance between the two points will agree. This is shown in the accompanying diagram. The one surveyor measures the lines AL and LB: the other the lines AL′ and L′B. Their systems of coördinates are different, but the distance between the two points, A and B, is the same.

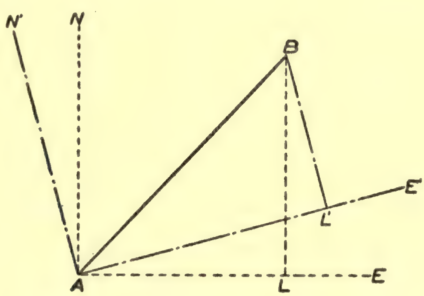


Fig. 5. Coördinates and Distance.

This distance, AB, may be found from the measurements of either surveyor by the aid of the well known problem of geometry, the problem which has been the bane of so many students since the time of Euclid, the pons asinorum: in a right triangle the square of the hypothenuse is equal to the sum of the

squares of the other two sides. The first surveyor measures the lines AL and LB, and from his work he concludes:

$$(AB)^2 = (AL)^2 + (LB)^2$$

and similarly the second surveyor finds:

$$(AB)^2 = (AL')^2 + (L'B)^2$$

The distance between the two points is the one fixed, invariable quantity: no matter how many surveys may be made, no matter how the various surveyors may run their lines, this distance, AB, comes out the same, provided only that the surveyors' work be accurate. This can be expressed mathematically by the equation,

$$D = \sqrt{x^2 + y^2}$$

where D represents the distance between the two points, and x and y, the measures by any surveyor, in any two directions at right angles to each other.

This relation, as expressed in the above formula, holds for any two points situate on a plane surface, upon the floor of a room, upon small portions of the surface of the earth. And an entirely similar relation holds for the distance between two points in space, between the diagonally opposite corners of a room, or between a point on the ground and the top of a church

spire. But in this case three measures are required, the length, breadth, and height of the room for example. And if we denote by z the third distance measured, then the distance between the two points will be given by,

$$D = \sqrt{x^2 + y^2 + z^2}$$

And this relation between the measured coördinates and the distance between the two points holds true no matter how the three lines, or coördinates, x, y, z, are run, provided only that they are mutually perpendicular to one another, like the three corner lines of a room. This equation, therefore, represents a definite, fundamental relation between the coördinates of points in ordinary space: the distance is the same, no matter upon what system the individual measures are made. In the terms of the mathematician, D is *invariant.*

Now Minkowski showed that, when the Lorentz transformation equations are used, there is a similar *invariant* quantity connecting the four coördinates, necessary to locate an event in space and time. This quantity is:

$$D' = \sqrt{x^2 + y^2 + z^2 - c^2 t^2}$$

where c is the velocity of light and t, the interval of time between the two events, and x, y, z, the ordinary three distance coördinates. Now Minkowski showed that, no matter in what directions the measures are

made no matter what system of coördinates be used, then D′ always has the same value; it is invariant, absolute, and thus furnishes a definite and fixed relation between the space coördinates and the time coordinate. It has been called the true or "absolute" interval between two events:—the interval in time and in space.

As this so-called relation between the measurements in the time and in space is one of the fundamental assumptions of the relativity theory, let us try to visualize it and see, if we can, what it really is. An event happens in New York City at ten o'clock in the morning of a certain day, a motor car accident at Fifth Avenue and 42nd Street for example; at four o'clock in the afternoon of the same day a second accident takes place at the same point in the city. These two accidents, happening at identically the same point in space (the motion of the earth being disregarded), are separated by a *time interval* only, by an interval of six hours. Now a third motor accident happens also at four o'clock, but in Washington instead of New York. This accident, or event, in Washington is separated from the second New York accident, not by a time interval, for they both occur at identically the same instant, but by a *space interval* or distance of some 250 miles. What is the actual, or absolute, interval in time and space between the first accident in New York and the third accident in Washington?

To this question the ordinary mortal, remembering his early lessons in elementary arithmetic, would answer that you cannot add time to distance any more than you can add horses to pigs, and that the two accidents are separated by a distance of 250 miles and by an interval of six hours in time. But the enlightened relativitist accomplishes the seeming impossibility by the aid of Minkowski's formula and finds something which he calls the space-time interval between the two events. He has either converted the horses into pigs, or the pigs into horses, or formed some hybrid out of them both.

This mathematical expression of Minkowski for a space-time interval corresponds closely to our ordinary expression for the distance between two objects, but not exactly. The term involving the time is preceded by a minus sign instead of a plus sign. The correspondence, however, can be made complete, if the time coördinate, ct, is replaced by the imaginary quantity $ct \times \sqrt{-1}$. Then the Minkowski expression becomes identical with the ordinary distance formula, except that it involves four quantities instead of three, and all the various equations of the relativity theory assume mathematical forms in four coördinates, in which this new time coördinate plays exactly the same rôle as the three space coördinates. But in all these expressions and equations the time coördinate involves a factor, $\sqrt{-1}$. This is the mathematical symbol for

an imaginary quantity, for something we can neither visualize, nor conceive of. It is useless to attempt to illustrate or visualize the connection between time and space; the very mathematical symbol used to denote the form of the connection indicates the impossibility of our doing so. Thus the very mathematical symbol, used by the followers of relativity, indicates the purely imaginary character of all their reasoning.

From these postulates and principles Einstein has built up his entire theory of relativity. These postulates and principles, it must be remembered, are pure assumptions, assumptions that may appear to have more or less plausibility, but assumptions nevertheless. But once these assumptions are accepted as true, it is possible to build upon them a complete and logical system of the universe and of all physical phenomena and happenings therein. This Einstein has done with great technical skill and with a broad view of the many intricate problems involved. In this development of his theories two stages are recognized, known respectively as the "special" and the "general" theories of relativity.

In the "special" theory, which antedated the "general" by several years, the motions of the reference bodies, or sets of coördinate axes, were assumed, or restricted, to be uniform, rectilinear, and non-rotary: that is, all were assumed to move forever in straight lines at constant speed and without rotation of any

kind. And the basic, underlying principle of the theory is that the laws of mechanics, of physics, all the general laws of nature, have exactly the same *form* when referred to every such frame of reference: that for the physical description of natural phenomena there is no "unique" frame, no body at absolute rest. Involved in this are all the special postulates, or assumptions regarding the velocity of light, the relativity of time, and the space-time theorem of Minkowski, all of which have been fully explained in the previous pages.

But in the universe at large there is no body which strictly complies with the restrictions of the special theory; no body, the motion of which is uniform, rectilinear, and non-rotary. All the bodies of the solar system are moving in curved paths and they are all rotating. To not a single body, therefore, can there be attached a set of axes, or a frame of reference, which fills the required conditions of the special theory. This theory is thus an approximation, a particular case; a case which can never actually occur, but which may be approached so closely in many physical problems of the laboratory, that the errors introduced through its use are negligible. But, when the motions of the planets themselves are considered in reference to the universe at large, then the restrictions of the special theory must be abandoned.

This leads up to the "general" theory, in which all

bodies are treated as being in gravitational fields and as having non-uniform, or accelerated motions. In this broader theory, the basic principle of relativity may be stated as: *"All bodies of reference K, K', etc., are equivalent for the description of natural phenomena (formulation of the general laws of nature), whatever may be their state of motion"* (72): or again, *"The general laws of nature are expressed through equations, which hold for all systems of co-ordinates."* This broader theory involves all the fundamental conceptions as to time and space as set forth in the "special" or limited theory, but the formulas, or equations expressing the results become more general or fluid in character.

Among all physical phenomena gravitation stands preëminent. Electric and magnetic phenomena depend upon the constituent material, and upon the physical state of a body, but gravitation is the same for all bodies and for all conditions of the same body; the same for lead and for a feather, the same for ice, water, and steam. A piece of lead and a feather fall towards the earth in exactly the same manner in vacuo, provided only that they start from the same point in space with the same initial velocity. Gravitation seems, therefore, an attribute of the particular point in space, rather than of the particular body which happens to occupy that point at any instant. According to Einstein the phenomena can be considered some-

what in the following manner:—a material body, the earth in the above example, affects the space in its immediate neighborhood, gives it a peculiar twist, or warp, produces, in other words, a gravitational field. The motions of any body entering such a field are determined by the laws which govern the properties in space of the gravitational field itself. What is to be considered, therefore, is not the motions of particular bodies, but the characteristics of space, as affected by the presence of matter.

Such study involves the most intricate mathematics, and the mathematical processes and methods, used by Einstein, cannot be explained in untechnical language. The general result, however, is that *"the geometrical properties of space are not independent, but they are determined by matter"* (135). By geometrical properties is to be understood the mathematical relations between measured quantities; between the radius of a circle and its circumference, between the area of a square and the length of its sides. Since the time of Euclid we have been taught to think that for every circle, wheresoever situated, on the earth, about the sun, near Venus, or in the vicinity of the North Star, the circumference is 3.141592+ times the radius. Not so in the relativity theory, every gravitational field has its own system of geometry. Near Venus this ratio may have one value, at the North Star an entirely different value. On the earth the area of a square, the

sides of which are two feet long, is four square feet. According to relativity the area of such a square, if transported to the sun, would not be four square feet, but something entirely different: in Orion this area would have a still different value. Thus, if the relativity theory be true, the formulas and methods of geometry and of engineering to which we are accustomed hold only for the Earth; the inhabitants of Mars, if any there be, have a different geometry and different formulas to solve their engineering problems.

Under the relativity theory the mathematical expression for the law of gravitation is not the same as that formulated by Sir Isaac Newton. This is necessarily so, for, in accordance with the principles of relativity, the velocity of a body enters into every formula and into every measure of its position in space. This was seen in the Lorentz transformation equations, which contain terms involving the ratio of the velocity of the body to that of light. The law of Einstein differs, therefore, from that of Newton by the introduction of terms depending upon this ratio of velocities. When the velocity of a body is extremely small as compared to the velocity of light, then this ratio becomes small and these terms become negligible in comparison with the other terms of the expression. In this case the formulas of Einstein degenerate into those of Newton, and, thus, for small velocities the two laws give identically the same results.

Besides this change in the law of gravitation, there are many logical deductions from the postulates and principles of relativity. Among the principal ones may be mentioned:

1. The *gravitational* mass of a body is equal to its *inertial* mass.
2. In gravitational fields there are no such things as rigid bodies with Euclidean properties.
3. Light is subject to gravitation, and in gravitational fields rays of light travel in *curved* paths.
4. The lines of the solar and stellar spectra are displaced towards the red end of the spectrum as compared with the spectral lines of the same element produced on the surface of the earth.

The following table exhibits a few of the principal differences between the so-called classical, or standard theories and the relativity theory, as enunciated by Einstein:

	Standard Theories	*Einstein Theory*
Space:	Independent & absolute.	Dependent: connected to and involved with time.
Time:	Independent & absolute: same everywhere and under all conditions.	Dependent upon space: varies with positions and motions of bodies.

Time intervals:	Identical everywhere and under all conditions.	Vary with the positions and motions of bodies.
Rigid bodies:	Of same dimensions and shape under all condi tions of motion.	Vary in size and shape with motion.
Geometry:	Laws and formulas the same everywhere and under all conditions.	Laws and formulas vary under gravitational action of material bodies.
Speed of light:	Constant in space.	Appears the same to every observer, whatever his motion.
Ray of light:	Travels in straight lines.	Travels in curved paths under attraction of material bodies.
Gravitation:	Independent of motion of bodies. Due to the attraction of material bodies.	Varies with the speed of bodies. Material bodies warp space, and this "warp" causes motion in bodies.

The fundamental postulates, or assumptions of relativity are so broad and general that it is next to impossible to directly test their truth or falsity. But,

indirectly the theory can be tested through conclusions, or formulas which have been logically derived from its basic principles. Einstein, himself, has devised certain physical and astronomical tests of his theories, and has claimed that such tests have been successfully met and conclusively prove the truth of the entire theory. Such claims and such tests are fully explained in the following chapter.

CHAPTER II

THE EVIDENCE FOR THE RELATIVITY THEORY

THE RELATIVITY THEORY strikes directly at our fundamental concepts as to the structure of the universe; its conclusions are startling and completely upsetting to our ordinary common-sense way of looking at physical and astronomical phenomena. To have such a theory accepted, it would seem that the evidence in its favor must be overwhelming, that the experiments, cited by its supporters, must be clear-cut and admit of no other solution. The burden of proof should be on the relativitist, and it should be clearly shown in each case or experiment, cited by him, that the relativity theory is the *necessary and sufficient* explanation; it should be established beyond all reasonable doubt, not only that the phenomena can be explained by the relativity theory, but that no other hypothesis or theory can equally well account for the observed facts.

Has this been done? Do the experiments and phenomena, cited by Einstein clearly establish the truth of his theories by excluding, as possible explanations, all other hypotheses and theories? In addition to the

"decisive" experiment of Michelson and Morley, Einstein claims four experiments, or observations, as fully and completely confirming his theories. Two of these might be classed as physical experiments, two as astronomical observations. These four are:

1. The Fizeau experiment on the velocity of light in a stream of flowing water.
2. The shift in the lines of the solar spectrum.
3. The motion of the perihelion of Mercury.
4. The deflection of light waves, as observed in the eclipse of 1919.

The first two of these so-called proofs are physical and belong primarily to the realm of experimental physics, to the realm of the laboratory; the latter two, on the contrary, are purely astronomical and must be studied and judged by astronomical methods. The astronomical observations are the ones relied upon, mainly, as furnishing the proof of the Einstein theories; that of the perihelion of Mercury being the first announced by Einstein and the one most widely quoted. The observations of the British astronomers at the 1919 eclipse, however, are equally important, are more striking, and more easily understood. It was the announcement of the results of these observations that caused the wide-spread, popular interest in Einstein and the relativity theory. These astronomical observations are fully explained and discussed in the following pages.

The physical experiments, or observations, on the other hand, have so far yielded little evidence for or against the theories of Einstein, and are, therefore, but very briefly treated. Sufficient outlines of the experiments, however, are given to enable the reader to form a judgment as to the character of the evidence and of the methods of reasoning adopted by the relativitist.

1. The Fizeau Experiment

The Fizeau experiment was first made in 1859. It has since been repeated in its original form and with modified and improved apparatus, with results always substantially the same as those obtained by Fizeau some twenty years before Einstein was born.

This experiment of Fizeau was a straight-forward, clean-cut attempt to verify certain predictions made by Fresnel as to the speed of light in different transparent substances, when the substances, or media are at rest and when in motion. These predictions had been embodied in what has come to be known as Fresnel's law: that the velocity of light in a moving transparent medium depends upon the speed with which the medium is moving and upon its index of refraction. This index of refraction, it will be remembered, is a definite optical property, or characteristic of a transparent medium; being, in fact, the ratio of the speed of a ray of light in vacuo to the speed of the ray in the

medium. This index varies for different substances, being different for air, for water, for glass, different for the various varieties of glass. Now in testing this law of Fresnel, Fizeau sent a beam of light through a stream of water, which was flowing through a tube with a definite and controlled velocity. He found that the velocity of such a beam of light is increased or decreased according as it travels with or against the stream, and he found that this increase or decrease, within the limits of accurate measurement, always bears the definite relation to the index of refraction and to the velocity of the water required by Fresnel's law.

Thus the results of Fizeau's experiments are in accord with the heretofore accepted laws of optics. Sir Oliver Lodge finds them to be in strict accord with classical ideas of a stationary ether and with the ordinary laws of optical phenomena. Lorentz also gave a satisfactory theoretical explanation of these results, basing his work on the newer ideas of the electromagnetic structure of matter.

The relativity theory can, however, also explain the results of these experiments. By using approximations and discarding certain small terms as negligible, Einstein succeeds in bringing his formulas into close accord with the observed facts, and in showing that these experiments do not invalidate his theories (48). But the fact that Lorentz had fully explained the

phenomena long before the relativity theory was formulated *"does not in the least,"* according to Einstein, *"diminish the conclusiveness of the experiment as a crucial test in favor of the theory of relativity, for the electrodynamics of Maxwell-Lorentz, on which the original theory was based, in no way opposes the theory of relativity. Rather has the latter been developed from electrodynamics as an astoundingly simple combination and generalization of the hypotheses, formerly independent of each other, on which electrodynamics was built"* (48).

These two sentences of Einstein are, from one point of view, as important as any in his work on relativity: —they should be read and re-read. They give a direct insight into his methods of reasoning. Here is an experiment, the details do not matter, an experiment claimed by Einstein as a "crucial test" of his theories, yet in the very sentence, in which this claim is advanced, he admits that other theories, the very theories he attempts to overthrow, can equally well explain the phenomenon. How can an experiment, equally well explained by several different theories, be a "crucial test" in favor of one of them?

2. Shift of Spectral Lines

According to deductions and calculations based upon the relativity theory, all the lines of the solar spectrum should be displaced slightly towards the red

end of the spectrum, when compared with similar lines obtained from terrestrial sources. It is well known that the light emitted by any element when rendered incandescent, consists of a series of waves of different lengths, and that, when such light is analyzed by, or spread out in, a spectroscope, these different sets of waves appear as separate bands or lines of light. Each element has its own characteristic series of lines by which it can be recognized, whether it be in the flame of a candle, in an electric arc, in the atmosphere of the sun, or that of a distant star. Under certain conditions these lines appear as bright streaks of color against a dark back-ground; under other conditions, as dark lines crossing the brilliantly colored spectrum. But, light or dark, the position of a particular line is the same, and it is the relative position of the line in the spectrum which identifies it.

The position of a line in the spectrum is not always absolutely the same: it shifts slightly towards the blue or towards the red end as the source of light is approaching towards or receding from the observer This is illustrated in the accompanying figure, which shows in the upper portion the brilliant sodium lines as produced in the ordinary spectrum on the earth, and in the lower portion the reversed spectrum of an imaginary star.

If the star and the earth were at rest relative to each other, then the dark lines of the stellar spectrum

would form exact continuations of the bright terrestrial lines. As, however, the stellar lines appear shifted

Fig. 6. Displacement of Spectral Lines.

towards the blue end of the spectrum, the star is moving towards the earth. The amount of the shift depends upon and is a measure of the speed of the approach of the star. Thus by comparing the lines in the spectrum of the sun, or of a star, with similar lines produced by an electric arc in the laboratory, the speed with which the body is approaching, or receding from, the earth can be measured. This fact, or principle, has been known for many years, and has been in constant use in determining the motions, to and fro, of the heavenly bodies.

Now Einstein has shown that, according to relativity, there should be an additional shift of the lines in the solar spectrum towards the red end of the spectrum. This Einstein shift is extremely small; the shift for each line being proportional to its wave length and amounting to about the two millionth part thereof. Thus, in other words, if the sun be approaching the earth, then the Einstein shift and the motion shift act in opposite directions and the total shift will

be slightly less than that due to the motion alone; while, if the sun be moving away from the earth, then the Einstein shift would be added to the motion shift, and the total would be slightly greater than that due to the motion alone. Thus the Einstein shift, if there be such a thing, is all tangled up with the shift due to the relative motion of the sun and earth; it is extremely small, is at the very limit of measurement, and can only be detected with the most powerful modern instruments and with the utmost refinements in method and care in making the observations. So difficult are the measurements and so surrounded by disturbing factors, that it seems hardly possible that a conclusive result can be obtained.

Some indications of an Einstein shift have been found by certain observers, but the shift so found, if real, does not agree in amount with that required by the relativity theory. Various lines in the spectrum give radically different results, and different observers find different results for the same line. While acknowledging these facts, Einstein announces his conclusions in the following words: "*Whereas Grebe and Bachem* (*Bonn*), *as a result of their own measurement and those of Evershed and Schwarzschild on the cyanogen bands have placed the existence of the effect almost beyond doubt, other investigators, particularly St. John, have been led to the opposite opinion in consequence of their measurements*" (158).

Here again is a typical example of the methods and reasoning of the relativitists. For, in considering this statement of Einstein, it should be remembered that St. John made his observations at the Mount Wilson Solar Observatory, with an equipment far surpassing anything to be found elsewhere, while the observations at Bonn were made with the ordinary, average equipment of a small laboratory or observatory.

3. The Motion of Mercury

In the motion of Mercury Einstein finds, however, a complete confirmation of the theory of relativity, a confirmation of the *necessity* as well as of its *sufficiency.*

It is well known and has been known for many years, that, in its motion about the sun, the planet Mercury exhibits a certain small irregularity, or discordance, the exact cause of which has troubled two generations of astronomers. The cause of this discordant motion, and a full explanation of it, is found by Einstein in the formulas of the relativity theory, and thus the relativity theory *"has already explained a result of observation in astronomy, against which classical mechanics is powerless"* (121).

Disregarding the action of other bodies of the universe, a planet, under the Newtonian formulas of classical mechanics, travels about the sun in an elliptical orbit, the major axis of which remains permanently

fixed in direction. Now in 1859 Leverrier found from observations of Mercury that, after due allowance for the action of the other known bodies of the solar system, the orbit of this planet does not remain stationary, but slowly rotates about the sun, as shown in the accompanying figure.

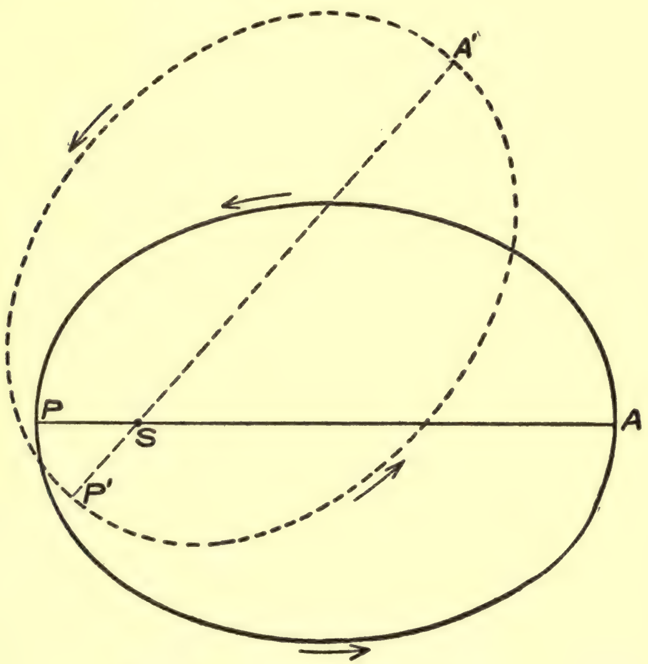


Fig. 7. The Rotation of Mercury's Orbit.

In this diagram the line PA is the axis of the elliptic path in which the planet travels about the sun, S. The point of closest approach to the sun, P, is called the "perihelion," and it is this point which it is cus-

tomary to use in fixing the direction of the axis PA. Now Leverrier found that, in the case of Mercury, the whole orbit is swinging around the sun, as on a pivot, so that after a lapse of ages the orbit will be found to lie in the position of the dotted ellipse, and the point P to have moved forward to the point P′. This motion is extremely small, the point P moving forward, according to Leverrier, only a few seconds of arc in one hundred years. It would thus require some thirty-three thousand centuries for the orbit to make one complete rotation.

At the time of Leverrier and for many years thereafter, this rotation, which has since been confirmed by Newcomb, was thought to be due to the action of a planet between Mercury and the sun, and the provisional name of Vulcan was given to such hypothetical planet. Long continued search, however, failed to locate Vulcan, and it is now recognized by all astronomers that Vulcan does not, and never did, exist. Other explanations of the observed rotation have been offered from time to time; but, according to Einstein, *"This effect can be explained by means of classical mechanics only on the assumption of hypotheses which have little probability, and which were devised solely for the purpose"* (123).

The contrary is the case with the relativity theory. Direct deductions from the Einstein law of gravitation, without the introduction of any new or special

factors, show that the orbits of all planets should rotate in the manner found for Mercury, and further show that, in the case of Mercury, this rotation should be at the rate of 43 seconds of arc per century.

This figure agrees almost exactly with that found by Newcomb in his first revision of Leverrier's work.

While theoretically, under the principles of relativity, all the other planets of the solar system should show similar orbital rotations, the actual amounts of such rotations decrease very rapidly with the increased distances of the planets from the sun. In all the other planets the magnitude of this motion is so small as to "*necessarily escape detection,*" except possibly in the case of Venus. The orbit of this planet, however, is almost an exact circle, which makes it more difficult to locate the perihelion with precision (152). Thus the orbits of all the planets, except that of Mercury, should remain apparently fixed in direction, should show no measurable rotation, or departure from the motions predicted by the Newtonian law. This according to Einstein "*has been confirmed for all the planets save one, with the precision that is capable of being obtained by the delicacy of observation attainable at the present time. The sole exception is Mercury, the planet which lies nearest the sun*" (122).

Thus the motions of the planets, according to Einstein, appear to confirm with remarkable precision deductions from the relativity theory and to show that

this theory approximates the truth far more closely than the Newtonian law of gravitation.

4. Curvature of Light Rays

That rays of light from distant stars are bent and deflected into curved paths, when passing near the sun, appears to have been proved by certain photographs taken in connection with the solar eclipse of May 29, 1919. It has been seen that Einstein's theories call for such a deflection. He predicted, in fact, many months before the eclipse took place that the deflection would amount to 1.75 seconds of arc for light rays just grazing the sun's surface.

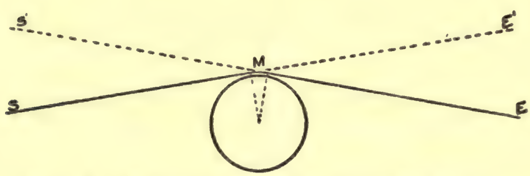


Fig. 8. Deflection of Light Rays by the Sun.

This is shown in the above diagram, where M is the sun and S and E, the star and earth respectively. The ray from the star starts out in the direction SME′ and, if the sun were not present, the star would be seen from the earth in the reverse direction, E′MS. The moment the ray enters the gravitational field of the sun, however, it is, according to Einstein, deflected into a curved path, being bent apparently around the sun. The light finally reaches the observer on the

earth at E, and to him the star appears to be at S′. The deflection is the angle EME′, and, as the intensity of the gravitational field decreases with increased distance from the centre of the sun, so the amount of this deflection will also decrease with the apparent distance of the star from the sun.

If in the course of its annual path through the heavens, the sun should chance to come approximately between the apparent positions of two stars, then would the rays from each star be deflected and the stars would appear at a greater distance apart than normally. On an ordinary night, when the sun is in another part of the heavens, two stars might appear as at S and S′, in the accompanying figure. When, however, the sun reaches the neighborhood of the stars at M, the rays from each will be deflected radially from the sun's centre, and the two will appear to be at D and D′ respectively.

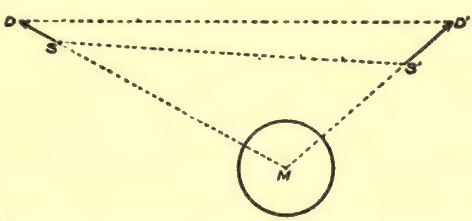


Fig. 9. True and Deflected Positions of Stars.

Now the sun is so brilliant that ordinarily stars in its immediate vicinity can neither be seen nor photographed. For the few moments of a total eclipse,

however, the moon cuts off all the direct sun-light, and, during this short interval, the stars may be photographed, and their respective positions determined from such photographs. These positions can be compared with standard positions, determined from similar photographs made when the sun was in another part of the sky.

In order to test in this way the prediction of Einstein and, through it, the relativity theory itself, The Royal Society and The Royal Astronomical Society equipped expeditions and sent them to Brazil and to the island of Principe, near the west coast of Africa. These expeditions made numerous photographs of the stars in the immediate vicinity of the sun during the eclipse of May 29, 1919. Similar plates were taken of the same stars, with the same apparatus, at other periods of the year when the sun was in another part of the heavens. The relative positions of the stars, as determined from the eclipse plates, were compared with the positions as determined from the second set of what might be called standard plates.

Such comparison showed clearly that the apparent positions of the stars, as shown on the plates, were different on the day of the eclipse from what they were when the sun was not in that portion of the sky. The expeditions appear to have proved the existence of light deflections caused by the presence of the sun. These observed deflections, however, did not agree

exactly with the prediction of Einstein, who had placed the expected deflection at 1.75 seconds of arc.

The report containing the results of the expeditions, as submitted to the Societies by the Astronomer Royal of England and the astronomers in charge, Eddington, Crommelin, and Davidson, shows that the mean result of the plates taken at Brazil gave a deflection of 1.98 seconds, that the plates taken at the island of Principe, under unfavorable conditions of cloud, gave 1.61 seconds. The conclusion of this committee of most able and noted astronomers is given in the report in the following words:

> *"Both these results point to the full deflection* $1''.75$ *of Einstein's generalized relativity theory, the Sobral results definitely, and the Principe results perhaps with some uncertainty."* *

According to Einstein himself, *"The results of the measurements confirmed the theory in a thoroughly satisfactory manner"* (115).

Thus two astronomical phenomena, radically different in character, appear to confirm the Einstein theories and deductions in a most brilliant and satisfactory manner. It would seem, therefore, as a direct result of astronomical research, that we must accept

* "A Determination of the Deflection of Light by the Sun's Gravitational Field," by F. W. Dyson, A. S. Eddington, and C. Davidson. *Memoirs, R.A.S.* 62.

the relativity theory, with all its implications as to warped space and variable time. On the other hand, the two physical phenomena, cited by Einstein, furnish practically no evidence in favor of the theory. Further refinements in method and advances in instrumental design may, however, enable physicists to determine the presence or absence of the displacement of the spectral lines, and thus either reinforce and confirm the astronomical evidence, or place the entire theory into the category of a brilliant, though unconfirmed hypothesis.

At the present moment the only tangible evidence in favor of the theory is that furnished by the motion of Mercury and the observed deflection of light. This evidence, certainly as stated by Einstein, makes a strong prima facie case for the theory of relativity; but, before the theory be accepted as proved, this evidence should be carefully examined in all its details. Many a well built-up case has completely collapsed when the evidence is sifted and the witnesses examined.

In the following pages the evidence in the case, Gravitation versus Relativity, as hereinbefore presented by the witnesses for Relativity, is subjected to a searching examination.

CHAPTER III

THE LAW OF GRAVITATION

THE LAW OF GRAVITATION, as announced in 1686 by Isaac Newton, is simply that every particle of matter in the universe attracts every other particle with a force proportional to the product of their respective masses and diminishing as the square of their distance apart increases. This law of attraction was deduced from experience, from experiment, and does not attempt, in any way, to explain how the force acts or why it acts; it merely states that such a force does act, and, in acting, follows a certain definite mathematical law.

This brief and simple statement of Isaac Newton summarized the experience of mankind from the earliest ages; codified and united the researches and experiments of philosophers and scientists from the days of Hipparchus. In the methods and discoveries of this most illustrious predecessor of Newton are to be found the basic principles of astronomical research and mathematical methods. Living in the second century before the Christian Era, he discarded the vague

speculations as to the primal cause of the movements of the sun, moon, and planets and substituted a study of the movements themselves; devised mathematical formulas and theories to represent these movements, to keep track of the positions of the bodies in the past, and to predict their places in the future. He invented the science of trigonometry, he elaborated the idea of the epicycle and used this powerful mathematical method for representing the motions of the sun and moon. Although the names, epicycle and epicyclic theory, have long fallen into disrepute, yet the fact remains that the mathematical method devised by Hipparchus under this name is still in use among astronomers and physicists, and forms the basis of the most modern tables of the motions of the sun, moon, and planets. The name has been changed, that is all.

As the epicycle has played, and still plays, such a prominent part in the theories of celestial motions, it should be thoroughly understood. It is merely a mathematical device for analyzing and keeping track of irregular motions, as is shown in the accompanying figure. Suppose a body, the sun for example, to travel at a uniform rate of speed in the circumference of the small circle, the centre of which is c. At the same time, this centre, c, is moving forward at a constant rate of speed in the large circle about E. In the first position, as shown in the diagram, the sun will appear from E to be in the direction of c;

but at a later time when S has completed one-half revolution, and c, one-quarter revolution in their respective circles, the sun will have reached the point,

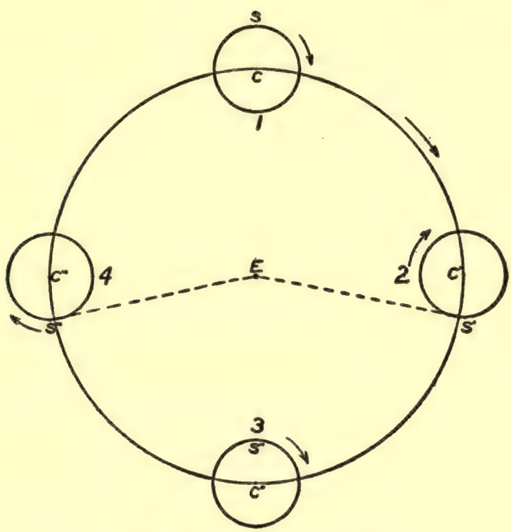


Fig. 10. Epicyclic Motion.

S′, and from the earth it will appear in the direction ES′, in advance of c′, as indicated in position 2. Thus during this quarter revolution the sun will apparently have been travelling faster than c. During the next quarter revolution of c, the sun will travel in the small circle to S″, and again the sun and c will appear in the same line, as viewed from E. In this quarter revolution the sun has apparently travelled more slowly than has c. Upon the completion of one entire revolution of c, the sun and c will return to their positions

which they held at the beginning and will again appear to be in a straight line, as viewed from E. Thus in the complete circuit, the average apparent speeds of the sun and c, as seen from the earth, are the same; but in the upper half of the path, as shown in the diagram, the sun will appear to be moving faster than c, in the lower half it will appear to be moving slower. The apparent variation in the speed of the sun as it revolves about the earth can, therefore, be explained as the resultant of two component motions, each uniform and circular. The larger circle, in the terms of the ancient mathematicians, is the deferent, the smaller circle, the epicycle.

By varying the relative sizes of the two circles and the respective speeds of revolution in each, very different types of resultant motion for the sun may be obtained. In fact, by a combination of a number of epicycles, by piling

"Cycle upon cycle, orb on orb,"

practically any irregular motion can be analyzed, or resolved, into a corresponding number of uniform circular motions.

The epicycle is not only a mathematical, it is also a mechanical, device, being used in many types of machines. As used by Hipparchus and his modern successors to describe the motions of the planets, it is a mathematical device, purely and simply, a com-

puter's fiction for keeping track of irregular motions, and did not and does not involve any idea as to the actual construction of the planetary system.

From and after the time of Hipparchus the varied motions of the planets were carefully studied, the crude tables of their motions gradually perfected and simplified. The development was extremely slow, each step in the process requiring centuries of effort. A few great names stand out—Ptolemy, Copernicus, Tyco and Kepler. Ptolemy was the direct successor of Hipparchus; he elaborated, extended, and perfected the methods of that more illustrious astronomer. He welded together all the observations and theories of his time into a compact and consistent theory of the universe; he formed and gave to the world in the year 150 A.D. a complete digest of astronomy, a digest so complete, so consistent, that it remained for over thirteen centuries the standard of scientific knowledge. He knew that the earth, taken as a whole, is a sphere, and he knew its approximate size; his tables represented the motions of the sun, moon, and planets with considerable accuracy, an accuracy truly remarkable for an age without means of accurate observations. Ptolemy, unfortunately, fell into the not unnatural error of thinking the earth to be at the centre of the universe, and the sun, the planets, and the stars as minor bodies revolving about it. This, of course, introduced complications into his mathematical tables,

but to him such complications seemed of less moment than the physical difficulties of a rotating earth. His cardinal error was in failing to recognize the atmosphere as a part of the earth and as partaking in any motions of the earth: to him a rotation of the earth from west to east would give rise to terrific winds, blowing at the rate of nearly a thousand miles per hour, hurricanes which would sweep the earth bare of trees and houses. It was for this physical reason that Ptolemy, disregarding the hints and suggestions of former writers, adopted his system of an immovable, non-rotating earth, with its unnecessary mathematical complications.

Not until the sixteenth century was there any radical change in the methods and theories of Ptolemy. Numerical corrections had been made, and his tables enlarged and improved; but fundamentally no changes had been made in the Ptolemaic system. In the popular mind the idea of the epicycle became fixed and the universe was thought of as consisting of a complicated system of revolving circles and spheres. The beauty of the purely mathematical computing device was lost in the attempt to realize the mechanism of the universe. The first great advance was made by Copernicus, whose book was published in 1543. He was struck with the unnecessary intricacies of the Ptolemaic system, with the utter impossibility of the innumerable stars revolving about the earth every

twenty-four hours. Through travel and voyages to distant lands a truer knowledge of the earth and the atmosphere had gradually developed, and Copernicus realized that the physical objections of Ptolemy to a rotating earth were not valid. He revived the older conception of Nicetas of a rotating earth, and of Pythagoras of a central sun, and combined them into a simple and beautiful system, which fully explained the larger motions of the sun and planets. In this system the sun is placed at the centre of the universe, and around it revolve the planets in circular paths, Mercury, Venus, the earth, Mars, etc.: the moon being recognized as a secondary body revolving about the earth. This approximates very closely to the true system of the planets. Copernicus, himself, realized that there are irregularities in the motions of the sun and planets, which his system, by itself, cannot explain, and he was forced to introduce a certain number of epicycles. By his rearrangement he struck out the larger epicycles of Ptolemy and reduced the number, but he still retained their machinery to explain, or account for, the smaller motions of the planets.

Before further advances could be made in the theoretical explanation of the planetary motions, these motions themselves needed a thorough investigation and a careful tabulation. For more than fourteen centuries there had been no improvement in the art of observing; the instruments of Hipparchus and Ptolemy

were still in use. The time was ripe for a new astronomer, one who would devote his entire lifetime to a diligent study of the heavens, one who would take no fact, no motion, on the authority of the ancients, but who would determine everything for himself. New instruments were to be invented, new methods of observing to be discovered. An observer, not a mathematician, was needed. This was the place filled by Tycho Brahe; he supplied the materials from which his successors built the structure of modern astronomy.

Tycho died in 1601 and left his uncompleted manuscripts and his observations to his student and follower—Kepler. Eight years later this illustrious astronomer announced his first two laws of planetary motion, and completed his discoveries nine years thereafter with the famous third law. In all he had devoted twenty-two years to his search, but he had solved the problem of planetary motions. By his discovery of these laws he swept aside all the old theories and machinery of the heavens, and laid the foundation upon which Newton built the wonderful edifice of universal gravitation.

These laws of Kepler summarized the observations of centuries, from the crude measures of the Chaldeans to the refined and then unsurpassed observations of Tycho. They are so fundamental to a clear understanding of the real motions of the planets and of the Newtonian law, that a full explanation is necessary

as to exactly what these laws are and as to what they actually mean. The first two laws describe the motions of a planet about the sun; the third law establishes a relationship between the orbits of the different planets. The three are:

1. The path of each planet is an ellipse, the sun being at one focus.
2. The line joining the sun and the planet sweeps over equal areas of space in equal intervals of time.
3. The squares of the periodic times of two planets (lengths of their respective years) are proportional to the cubes of their mean distances from the sun. (One-half of the sum of the greatest and least distances.)

The exact meaning of these three laws is shown in the annexed figure:

A planet, Mercury for instance, travels about the sun, S, in the mathematical curve commonly called an oval, but technically known as an ellipse, and shown in the diagram as ACBD. This curve is not placed symmetrically with respect to the sun; it is off centre, so to speak, and the sun lies at a point called the focus. In this curve the planet travels at varying speeds; when near the sun it travels considerably faster than when at a more distant part of its path, and this variation in speed is the subject of Kepler's second law. In a given interval of time, one week for example, the planet will travel from A to C, and the line join-

ing it to the sun will, during this interval, sweep over the area ASC. Some time later, in one week the planet will travel from B to D and Kepler's law states

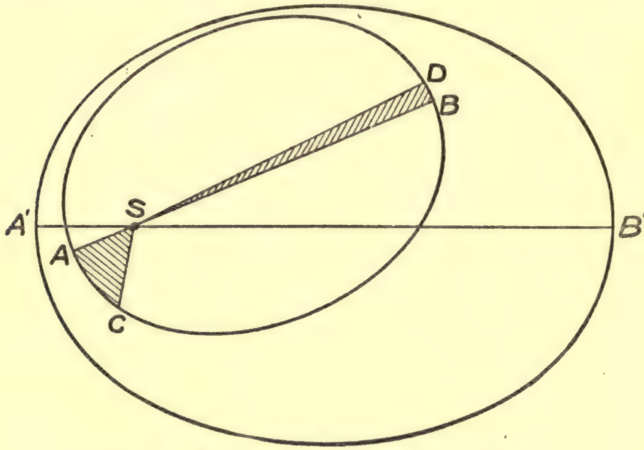


Fig. 11. Kepler's Laws of Planetary Motion.

that the distance thus travelled is so proportioned to the former distance AC, that the area SBD is equal to the area ASC.

These two laws fully describe the motion of the planet, and enable one to keep track of its movements in the heavens and to predict where it may be seen at a given time. They did away with the need of epicycles; the irregular motion of the planet, its variation in speed and direction, were fully accounted for by the shape of the path and the varying speed of the planet in that path.

The third law expresses a relation between the orbits

of two planets; between the size of the orbits and the length of time required for each planet to complete one trip around its oval path.

Now these laws of Kepler sufficed to trace fully the paths of the planets, as shown by the observations of that date. The telescope was not then invented, and the observations were still crude and approximate in comparison with modern accuracy. Irregularities of motion, too small to be detected in the observations of Tyco, are now known to exist, and the laws of Kepler are now known to be but approximations, but remarkably close and accurate approximations.

While this line of eminent astronomers were studying the heavens and analyzing the varied motions of the planets, other scientists were studying the laws of terrestrial mechanics and inventing methods of research and calculation. The device of logarithmic computation doubled or quadrupled the effective working life of an astronomer, and allowed computations to be made, which otherwise would have been hopeless from their very length and complexity. In the realm of the physical sciences the name of Galileo stands preeminent. He laid the foundation of the modern science of mechanics, he formulated the fundamental laws of motion, discovered the law of acceleration of falling bodies, and determined the path of a projectile. He invented the telescope as an instrument of astronomical research, and, by its use, discovered many facts that

reinforced and proved the general correctness of the Copernician system—the rotation of the sun on its axis, the phases of the planets, and the satellites of Jupiter.

Newton, the greatest, the most eminent scientist of all ages, brought together the physical discoveries of Galileo and the astronomical laws of Kepler, and united the whole mass of unrelated laws and phenomena into a single, all comprehensive, fundamental law—the law of universal gravitation. The laws of Kepler were shown by Newton to be the inevitable result of a force of attraction, directed towards the centre of the sun and diminishing in intensity proportionally as the square of the distance therefrom increased. This force keeps the planets in their orbits and directs their movements. Reasoning by analogy, it was easy for Newton to infer that the moon was kept in her orbit by a similar force directed towards the centre of the earth. Bodies on the surface of the earth fall freely, acted upon by a similar force. But are the two forces, the force that keeps the moon in her distant path and the force that causes a stone to fall, are these two forces one and the same? Newton thought so, and in 1665 tried to prove his hypothesis. The stone falls towards the earth 16.095 feet in a single second of time. And if the force, which causes it to fall, decreases as the square of the distance from the earth's centre, then at a distance of 60.3 times the radius of

the earth the stone would fall only 1/3636th as far. At this distance, which is the average distance of the moon from the earth's centre, the stone would, therefore, fall only about one-twentieth of an inch, actually 0.0532 inches.

Now, if the force that keeps the moon in her orbit is the same as that acting on the stone, then the moon should be falling towards the earth by exactly this same amount, namely 0.0532 inches, in one second. The amount that the moon actually does fall is the amount by which it is deflected from a straight line, as it travels in its orbit and as shown in Figure 12.

If it were not for the attraction of the earth, the moon would move in the straight line indicated by the arrow AD. The pull of the earth, however, causes the moon to travel in the curved orbit AB. In one second of time the moon moves over the arc AB, and consequently falls towards the centre of the earth by a distance represented by the line AC. This distance can be calculated as soon as the length of the arc AB is known together with the actual size of the moon's orbit.* The angular value of this arc can readily be computed, for it takes the moon some 27 days 7 hours and 43.2 minutes to make one complete revolution

* Considering the arc AB as a straight line and the triangle ABF as right angled, we have

$$AC : AB = AB : AF \text{ (or } 2r)$$

whence:

$$AC = \frac{(AB)^2}{2r}$$

about the earth. Reducing this to seconds, the sidereal period of the moon is 2,360,591 seconds and, therefore, the arc AB is 1/2,360,591th part of a circum-

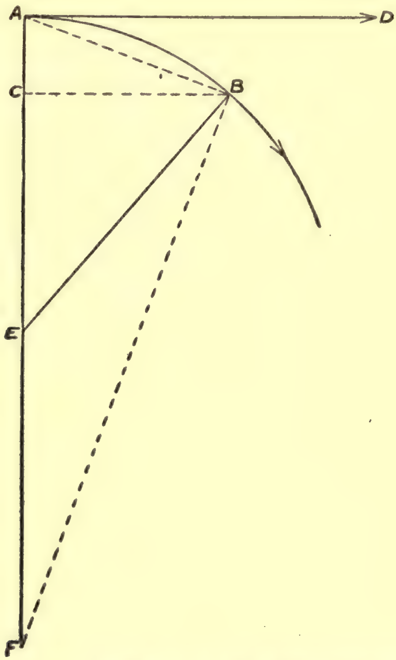


Fig. 12. The Fall of the Moon towards the Earth.

ference. On the other hand, to find the size of the moon's orbit the actual dimensions of the earth in feet must be known, for the radius of the orbit is 60.3 times the radius of the earth. In 1665 the exact size of the earth was not known, the length of a degree on the surface was supposed to be sixty (60) miles,

which made the radius of the earth 18,150,000 feet. With these figures the distance AC through which the moon falls in a single second comes out as 0.0457 inches, which differs by about 15% from the 0.0532 inches found for a stone at the moon's distance from the earth.

This difference of 15%, or a trifle less than one one-hundred and thirtieth (1/130th) of an inch in the motion of the moon, caused Newton to consider his theory as *not proved* and he laid aside his work. Nearly twenty years later the measurements of Picard, on the length of a meridian arc, were brought to Newton's attention by Halley, who urged him to take up again his discarded work and to revise his calculations. These measurements showed one degree to be somewhat over 69 miles in length, instead of the even 60 used by Newton in his former calculations. The true length of the radius of the earth was thus shown to be nearly twenty-one million feet (20,926,000) instead of the eighteen million used by Newton in his first calculations. With these new figures, the distance that the moon falls towards the earth comes out as 0.0535 inches, which agrees with the figure derived from the falling stone to within less than one one-thousandth of an inch (3/10,000), or to within three-quarters of one per cent.

Thus Newton in 1685 proved that the force, which retains the moon in her orbit, is the same force, which

causes bodies to fall to the earth, and that this force, gravitation, varies inversely as the square of the distance from the earth's centre. The sun attracts the earth and its satellite, the earth attracts the moon, and, unless the attraction of the earth ceases at some point, the earth must also attract the sun and all the planets as well. Thus the earth attracts each and every particle of matter, the sun attracts each and every planet and satellite, and in turn the planets and satellites attract the sun. Thus did Newton infer the law of universal gravitation.

The law of gravitation is thus an empirical law; it is deduced from experience. So far as is known to-day, gravitation is a fundamental property of matter. The action of the force is instantaneous, it is not modified, in any degree, by the interposition of other particles of matter, and it is independent of the structure and condition of a body: it is the same for ice, water, and steam.

As enunciated by Newton, this law deals with individual particles of matter, separated by finite distances. It does not deal with forces of cohesion, or with the molecular forces which may determine the physical condition of a body. For separate particles of matter the mathematical statement of the law is very simple.

Particle A, for example, attracts particles B, C, and D, and is in turn attracted by each and every one

of those particles. The amount of this attraction depends in each case upon two factors; the product of the masses of the two particles under consideration,

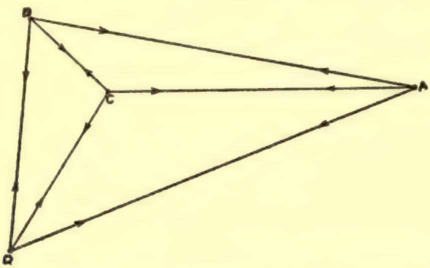


Fig. 13. Mutual Attractions of Particles of Matter.

and their distance apart. Thus the force between A and B is measured by the mass of A multiplied by that of B, and the product divided by the square of the distance between A and B. At double the distance apart the force is one-quarter, at ten times the distance, one one-hundredth. And this law holds for individual particles no matter what the distance between A and B, whether measured by the thousandth of an inch, or by millions of miles.

But a body of material size is made up of a collection of particles, more or less firmly bound together. That is, if the particles B, C, and D are brought together at C, they might be considered as forming a single body, but as these three particles cannot occupy the same space at the same time, the actual distances of the three from A will still differ by minute quantities, and the total

force acting upon A must still be determined by taking into account these differing distances. Thus, while the mathematical expression of the law is very simple when stated in terms of particles, it becomes complicated when applied to actual material bodies. In fact, it becomes impossible, except in one or two simple cases, to find the exact mathematical expression for the attraction between two bodies.

The great exception is for bodies of a spherical form. The force of attraction between two homogeneous spheres of matter, two balls of iron or of copper, can be expressed with the simplicity of that for two particles. Such spherical balls of matter act as though their entire masses were concentrated at their respective centres:—the only distance that enters the formula is the distance between the two centres. Similarly for a shell of spherical form; it acts as if all its constituent particles were united into a single particle at the centre. Thus a body made up of a series of spherical shells of different densities, or even of different materials, acts as though the entire mass were concentrated at the centre, the only condition being that each shell be uniform in itself. That is, the law of attraction can be very simply, but rigorously, expressed for solid spheres of copper, silver, or lead; for thin spherical copper vessels filled with water, or with melted lead; for a sphere made up of concentric layers of copper, iron, gold, and lead. It makes no difference whether the denser material be

inside or outside, provided only that each spherical layer be uniform.

If, however, the shape of the ball be changed to an ellipsoid, or egg-shaped body, then at once the simple expression for the attraction vanishes, and is replaced by a most complicated formula. If the body be non-symmetrical, or irregularly shaped, then it becomes impossible to find a complete mathematical expression for the force of attraction it exerts upon another body. This force, of course, is a certain definite quantity, but we cannot express that quantity in definite mathematical language. In other words, if we have two bodies, both made of copper of uniform density, one a perfect sphere two feet in diameter and the other an irregular lump, we can compute exactly the attractive

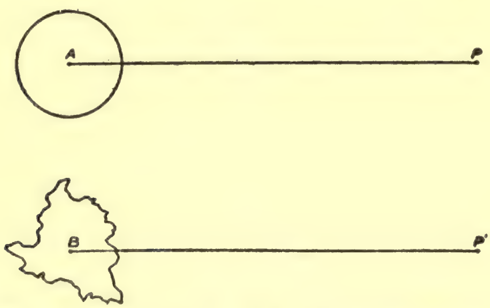


Fig. 14. Attractions of a Sphere and of an Irregular Body

force of the sphere, A, but we cannot, except very roughly, determine mathematically that of the lump, B.

An analogous, but not exactly similar, case might be

used as an illustration. If we have a sphere, or a cube, of copper of known density, we can readily measure with calipers the diameter of the sphere or the side of the cube, and then by perfectly simple mathematical formulas we can compute the volume of the sphere or cube, and then from the known weight of a cubic inch of copper we can calculate the exact weight of the body. But if we have a lump of copper, irregular in shape, with excresences sticking out in all directions, it would be well-nigh impossible, by any system of measurements, to determine its exact volume and weight. Of course, in this case the weight of the lump could be at once determined by a pair of scales, but not by mathematical calculation.

The sphere is the only type of celestial body of which the force of attraction can be rigorously calculated. For bodies of any other shape, approximations must be used.

There is a large class of bodies, variously known as ellipsoids, or spheroids, some of which differ very little from spheres, and this class is of importance in the discussion of the planetary motions. The important type of bodies are those formed by the revolution of an ellipse about one of its axes. If in the figure the ellipse ANBS be revolved about the axis NS, it will develop a solid, such that every section perpendicular to this axis will be a circle, every section through the axis will be an ellipse exactly similar to the one shown,

and every other section, an ellipse intermediate between the circle and ANBS. The small circle, inscribed in the ellipse, will develop a sphere. Now, it is a very simple

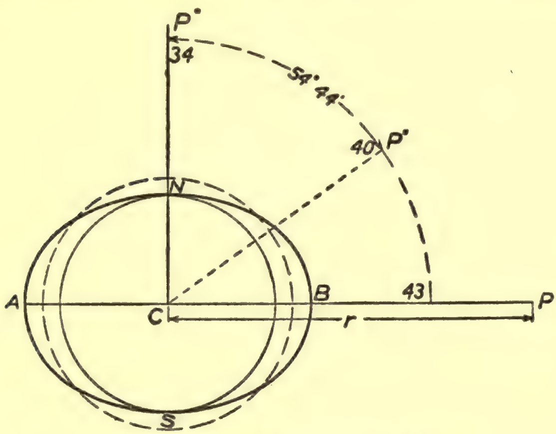


Fig. 15. Attractions of Spheres and Spheroids.

problem in geometry to find the volumes of the sphere and of the ellipsoid. If we know the density of the material, we can find the total amount of matter in the spheroid just as easily as we can that in the sphere, and we can then find the respective weights of these bodies, with an equal degree of accuracy in each case. A sphere, shown by the dotted line, can also be found, such that its mass, or weight, is exactly equal to that of the spheroid, M.

But the case is different, when the attractions of these bodies upon a particle of matter at P is required. Each of the spheres attract P as though their entire masses

were concentrated at the centre, c. If the mass of the particle, P, be taken as unity, the attraction of the equivalent sphere is

$$\frac{M}{r^2}$$

But it is clear from the figure that this is not so for the spheroid, for quite a bulge of matter at B is nearer to P than any of the matter in the sphere, and a similar bulge at A is farther away. While these bulges are symmetrically placed as regards the sphere, yet that at B is much nearer to P than that at A, and will, therefore attract P more strongly, and so it is easy to see that, as a whole, the spheroid will attract P more strongly than will the equivalent sphere. The amount of this excess attraction over that of the equivalent sphere depends upon two factors; first the shape of the spheroid, upon its eccentricity in other words, and second, the relative distance of P from the centre as compared to the size of the spheroid, or upon the ratio of CB to CP.

If the point P, instead of being in what might be called the equator of the spheroid, were in the prolongation of the axis, or directly over what might be called the north pole, then it is easy to see from the figure that the equivalent sphere would attract P more strongly than the ellipsoid. The attraction is the least when P is in the prolongation of the axis, greatest when in the

so-called equator. At some point midway between these two, the attraction of the spheroid will exactly equal that of the equivalent sphere. This point is 54° 44′ from the axis. The attraction of the spheroid upon P varies, therefore, not only with the distance of P from the centre, but also with the direction of P with respect to the axis of rotation. It is thus impossible to find a simple and accurate mathematical expression for the attraction of the spheroid upon a particle in space. The very best that can be done is to express this force in a series of terms, the first of which is the attraction of the equivalent sphere, the following terms growing smaller and smaller. This expression is

$$\frac{M}{r^2}\left[1 + \frac{3}{5}\,\varepsilon\,(1 - 3\,cos^2\,\gamma)\ \frac{a^2}{r^2} + \ldots\right]$$

in which ε is the ellipticity of the spheroid and γ, the angular distance of the particle P from the axis NS. To fully appreciate the meaning of this formula, consider the specific case in which the shorter axis of the figure is just one-half the longer, and find the attraction of the spheroid upon particles, distant from the centre just twice the longer axis of the body. In this case ε is equal to ½, as is also the ratio a/r. For a particle, P′, in the equator of the body γ is 90°, and *cos* γ is zero, and the expression reduces to:

$$\frac{M}{r^2}\left[1 + \frac{3}{10} \times \frac{1}{4}\right]$$

and the whole attraction of the spheroid upon P′ becomes,

$$\frac{43}{40}\text{ths}$$

that of the equivalent sphere.

Similarly for a particle, P‴ above the north pole, the expression becomes:

$$\frac{M}{r^2}\left[1 - \frac{6}{10} \times \frac{1}{4}\right]$$

or

$$\frac{34}{40}\text{ths}$$

that of the equivalent sphere.

The particle at P″ will be attracted with a force exactly equal to that of the sphere. Thus it is seen that for particles situated at the same distance from the centre of the ellipsoidal body, the force of attraction varies by 9/40ths of its total amount, by 22 ½%, varying thus with mere changes in direction. These results, however, are not fully accurate, for only one term of the expression for the force has been taken account of: there are in reality many other terms, but these are all small in comparison with the one used.

But as the distance of the particle from the body increases, the total attraction approaches more and more closely to that of the equivalent sphere. This

approach is very rapid, for the expression for the force depends upon the square of the ratio a/r. In the above special example, if the particle were removed to a distance equal to ten times the longer axis of the spheroid, then the force of attraction of the spheroid would differ by less than ½ of one per cent from that of the sphere of equal mass: at a distance of one hundred (100) radii, this difference of attractions for the sphere and the spheroid would be considerably less than one one-hundredth of one percent.

If it is thus impossible to find a rigorous mathematical expression for the attraction of such a simple body as an ellipsoid, how utterly impossible must it be to find any expression for the attraction of an irregularly shaped body! Approximations must be used, and the first, the FUNDAMENTAL APPROXIMATION used in all astronomical researches since Newton announced the law of gravitation, is the assumption that all the bodies of the solar system are homogeneous spheres.

This assumption is necessary, it cannot be avoided. It may approximate very closely to the truth, but it is an approximation, and any motions, or laws of motion, derived by the use of this assumption are *necessarily approximations.*

Fortunately there are two circumstances, which make this approximation allowable in treating of the motions of the planets:—the bodies of the solar system differ

but little from spheres, and their mutual distances apart are very great as compared to their individual dimensions. Saturn differs more greatly from a sphere than any other known body of the system. Viewed in a telescope, its disc is noticeably elliptical, and, in fact, its ellipticity is found to be almost exactly 1/9th. Jupiter also shows a disc distinctly not circular, and measurements indicate its ellipticity to be 1/17th. All the other bodies approach much more closely to true spherical forms; the ellipticity of the earth is but 1/293th while that of the sun is extremely minute. But, on the other hand, the distances between these bodies are relatively very great. At her nearest approach to the earth, Venus is distant over six thousand times the radius of the earth, and the excess attraction due to the ellipticity of the earth amounts to only about one twelve-billionth that of the whole force beween the two bodies.

It is different, however, when the motions of the satellites are considered. As early as 1748 Euler showed that the spheroidal figure of Jupiter would cause irregularities in the motions of his satellites, and ten years later Walmsley showed that the elliptical shape of Jupiter would cause a rotation of the orbit of each satellite, *a rotation exactly similar to the now much discussed motion of the perihelion of Mercury.* The ellipticity of the earth affects the motion of the moon to a very noticeable amount, and such ellipticity is taken

account of in all theories of the moon's motion and in all tables from which her place in the heavens is predicted.

The planets, as has been seen, are so small relative to their mutual distances apart, that their individual shapes can have no appreciable effect upon their motions. Not so, however, with the sun; its dimensions are appreciable fractions of the planetary distances. Mercury is distant only eighty (80) times the solar radius, the earth two hundred and fifteen (215). The distance factor in the expression for the excess attraction due to the shape of the central body is thus for the case of Mercury 1/6400; for the earth 1/46,000. If, therefore, the sun be not strictly spherical, the variation in the force of gravitation due to its shape may have a measurable effect upon the motions of the nearer planets.

In all the planetary theories and computations the sun has been regarded as a sphere, and all influences due to a possible departure from such shape have been omitted. From the time Galileo first turned his puny telescope on the sun and discovered its surface covered with dark "spots," it has been known that the sun's density is not uniform; from the time Newton evolved his law in 1686, it has been recognized that the sun is not a sphere of uniform density, and that, in neglecting all questions as to its true shape and condition, a fundamental and far-reaching approximation was used.

In the earlier days of celestial mechanics, when observations were still crude, the use of this approximation was fully justified, whether it be so today is a moot question. During the last century numerous series of measures have been made to determine, if possible, the exact shape of the sun.

Every series of measures, heretofore made, shows a distinct, measurable departure from sphericity, but these departures are extremely minute. Such measures are extremely difficult to make, for the visible edge of the sun is not a distinct, clean-cut line; it is hazy, indefinite, and fades out gradually. Further, the atmosphere of the earth, through which the light comes from the sun, is seldom in a state of absolute rest, and the movements of the air currents cause apparent tremblings or "boilings" of the telescopic images of sun and stars. This unsteadiness of the atmosphere is more marked by day than at night; this being due probably to the heating effects of the sun's rays. Photographs of the sun are made with exposures of about 1/1000th of a second, yet even on such photographs the "boiling" is noticeable, and only at rare intervals will a perfect plate be obtained.

During the first half of the nineteenth century there were many visual observations, or measurements of the size and shape of the sun. The results were not very accordant, differences in the lengths of the polar and equatorial radii amounting to four (4) and five (5)

seconds of arc were found; some observers finding the polar radius the longer, others, the equatorial. Somewhat later, Auwers collected and discussed all the observations, made at the leading observatories of the world, Greenwich, Washington, Radcliffe, and others; something like 30,000 observations made by nearly one hundred different astronomers. From this immense mass of material he finally concluded that a polar compression of 1/4,000th, or an excess of 0″.5 of the equatorial over the polar diameter, would best represent all the observations.

The transits of Venus across the disc of the sun in 1874 and in 1882 furnished splendid opportunities for determining the size of the solar disc. An extremely accurate measuring instrument, the heliometer, had been devised and elaborate preparations for observing the transits were made. During the preparatory work of adjusting the instruments and determining their constants, the German astronomers made many long series of measurements of the solar diameter. Five instruments were used, measurements with the same instrument being made in various localities by the same observer, and at the same station by various observers. In all some 2692 separate measures of the sun's diameter were made by twenty-six (26) observers. This mass of data was thoroughly discussed by Auwers * in

* Astronomische Nachrichten, vol. cxxviii, No. 3068, December, 1891.

1891, with the conclusion that the polar diameter exceeds the equatorial by the very minute amount of 0.038 seconds of arc. This result is contrary to what would normally be expected. The sun is a rotating gaseous body, and, under all laws of physics and mechanics, the equatorial diameter should be the longer by at least 0″.05. Newcomb,* however, refers to this investigation of Auwers as completely setting at rest all questions as to the sun's shape, and concludes that there can be no non-symmetrical distribution of matter in the sun sufficient to cause any appreciable variation in the motions of any planet.

A result almost identical with that of Auwers, was obtained by Schur and Ambronne in 1905. They made a series of measures of the sun's diameter with a six-inch heliometer during a period of nearly thirteen years, from 1890 to 1902, finding that the polar diameter exceeds the equatorial by 0″.025. This result, as well as that of Auwers, represents the mean or average of all the observations, regardless of the time at which they were made. The individual results varied in different years; in some years the polar diameter appeared the greater, in other years, the equatorial.

This question as to a possible variability in the shape of the sun has also been made the subject of many investigations. It is a well-known fact, easily confirmed by any amateur observer, that the visible "spots" on

* Astronomical Constants, Washington, 1895.

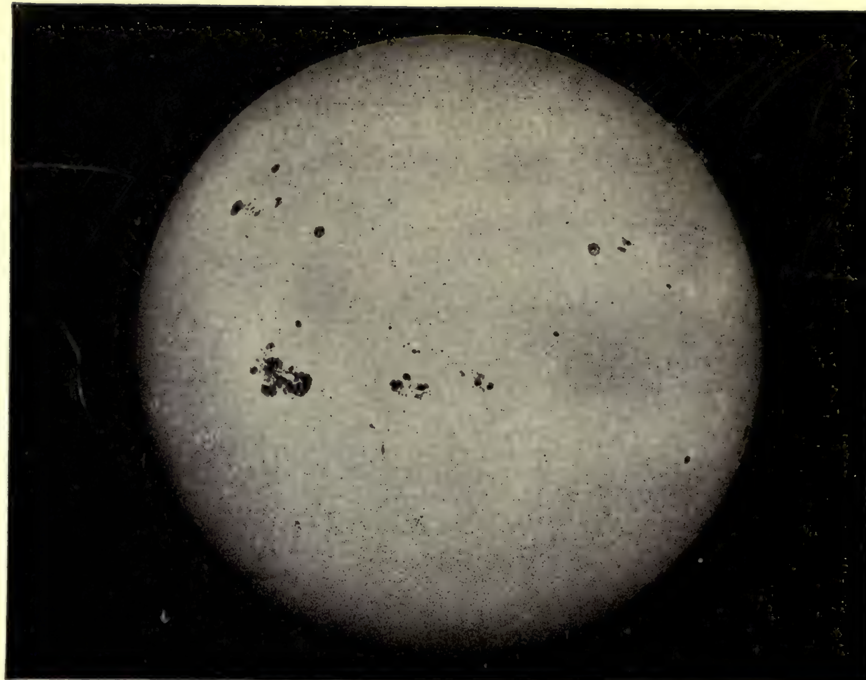


Plate 2.

The Surface of the Sun: a photograph taken at the Mount Wilson Observatory.

In mathematical calculations the Sun is assumed to be a sphere, each layer of which is of uniform density. The immense spots clearly show that this assumption is not valid, and that any conclusions based upon this assumption cannot be strictly true.

the sun's surface are periodic in character; their average number waxes and wanes in regular periods of eleven years. During a minimum, practically no spots are visible, days and weeks often passing without a single spot marring the brilliant solar surface. Then a few small spots appear, and gradually the number and size of the spots increase, until after the lapse of five and a half years portions of the surface are constantly covered with large and small spots. Occasionally spots are so numerous at times of maxima, as to form two great belts around the sun, one on each side of the equator. Now, if this eleven-year cycle is a natural period of the sun, due to its physical condition as a rotating, cooling mass of gas, then it is highly probable that it expands and contracts as the number of spots wax and wane; and this expansion and contraction would be reflected in the measured values of the diameter. Practically every series of measures, heretofore made, show periodic variations in the size and shape of the sun; but it has not been proved that such variations agree in period with the sun-spots.

In his investigation of the long series of observations, heretofore referred to, Auwers first found that the measured diameter of the sun varied with the number of visible sun-spots. He afterwards revised his discussion and assigned variable "personal equations" to the different observers; making those of some observers vary with the time, and those of other show abrupt

changes. With such set of variable personal equations, Auwers was enabled to reduce the apparent variation in the observations, so that all semblance of periodicity disappeared. In other words, it appeared more plausible to Auwers that a dozen or more observers should each change his personal habit of observation by just the right amount and at exactly the right time, rather than have a real change in the object they all were measuring. This may be the true explanation of the variations in the measured diameter of the sun; but it is evident that it is perfectly possible to get any pre-determined result in this way.

On the other hand, Ambronne found a small periodic variation of about $0''.1$ from his own long series of measures, but this variation did not appear to bear any relation to the sun-spot period. A revision* of this work of Ambronne, together with an investigation of the transit of Venus observations, confirms a variation of about the size found by Ambronne, but differs from his conclusion, in that it indicates a close connection between the period of this fluctuation and that of the sun-spots. Some faint indications of short period fluctuations were also found; periods varying from ten hours to twenty-eight days.

Thus all the measures made of the visible surface of

* *An Investigation of the Figure of the Sun and of Possible Variations in its Size and Shape,* by Charles Lane Poor: New York Academy of Sciences, vol. xviii, No. 9.

the sun show that it is almost exactly a spherical body, but indicate that its shape is subject to minute fluctuations. The average difference of the diameters of the sun cannot be more than a small fraction of a second of arc, and the amplitude of the fluctuation is also extremely minute, being probably not over a tenth of a second of arc. Such minute departures from sphericity are far too small to have any appreciable effect upon the larger motions of the planets, and the fundamental approximation of Newton and his successors in regarding the sun as a sphere is thus justified for the relatively crude observations of those times. But the sun, clearly, is not a sphere of uniform density, and the departure from sphericity, minute though it may be, must be taken into account when considering the intricate motions of the inner planets. Any deductions, obtained through the omission of this non-sphericity, are clearly approximations; extremely close approximations, but approximations nevertheless.

The SECOND FUNDAMENTAL APPROXIMATION in all planetary theories is that space is empty; that, so far as the equations of motion are concerned, no bodies exist other than the sun, the planets, and their satellites. In every mathematical discussion of the motions of the planets, in the works of La Place, Leverrier, and of Newcomb, the solar system is considered as formed of a limited number of material

particles of certain masses, each of which attracts every other one according to Newton's law of gravitation. The first of these material particles represents the sun, the second, Mercury, the third, Venus, the fourth, the earth-moon system, and so on to Neptune and his satellite. The planetoids, the comets, the matter forming the solar corona, and any and all other matter, which may lie about, or between the planets; all these are considered as absolutely negligible. All the tables of planetary motion, all the formulas and deductions of celestial mechanics are based upon the assumption that these smaller bodies, and this scattered matter, have and can have no effect upon the motions of the planets. In a first approximation to the planetary motions, this assumption is essential; the equations of celestial mechanics would be unworkable if an attempt were made to introduce factors representing these bodies. As will be seen in a subsequent chapter, the mathematical difficulties in tracing the paths of even a limited number of bodies, are almost unsurmountable; to attempt to formulate theories of motion of a great number of heterogeneous bodies of all sizes and conditions would be hopeless.

A comparison of the calculated motions of the planets with their observed paths shows that this approximation is justified. The larger motions of the planets are exactly represented; the more minute variations in motion are represented with extreme accuracy.

This assumption of empty space serves, thus, not only as a first, but as a second approximation. But it is an approximation, and these neglected bodies must have some effect upon the motions of their larger fellows, and such effect must, in the course of time, appreciably alter the motions of one or more of the planets.

Now consider for a moment what these neglected bodies really are, so as to have some idea as to the order of this fundamental approximation. Comets may be dismissed at once from consideration; they are erratic wanderers, and any effect that they might have upon the motions of the planets, therefore, would be momentary. There is not the slightest indication that a comet has ever affected, in any measurable degree, the motion of even the smallest planet or satellite.

The sun, however, is surrounded by an envelope of matter, an envelope which becomes visible at times of eclipse. This brilliant halo, the Corona, has been known from the times of remotest antiquity as one of the most beautiful of all natural phenomena. Since the days of exact science it has been drawn many times, it has been photographed times without number. In shape and brilliancy it is extremely variable, although certain general characteristics are maintained from eclipse to eclipse. The inner portion appears as an almost continuous ring of brilliantly glowing matter, while the outer portions are made up of radiating bands and filaments, which vary greatly in length. The longest

streamers are usually found in connection with those portions of the solar surface to which the sun-spots are confined, and some indications point to a real connection between the shape and size of the corona and the sun-spot period. At times the coronal bands and streamers have been traced to great distances from the disc of the sun,—ten or more diameters. In a very general way it may be said that the corona is lens-shaped,—an ellipsoid of matter with its longer axis in, or near, the plane of the sun's equator.

The total light of the corona is two or three times that of full moon, and such light is found to be made up, partly of reflected sun-light, partly of light from a self-luminous gas. Thus, the corona must consist of a great mass of incandescent gas, intermixed with small particles of solid or liquid matter. Very little, however, is known as to its physical constitution: it is generally assumed to be of inconceivable rarity, its density far less than the best vacuum produced in our laboratories. This is purely a matter of speculation; nothing definite is known as to its actual density, beyond the mere fact that it is small as compared to the earth's atmosphere. The conditions in the immediate vicinity of the sun are so radically different from anything known on the earth that all analogies fail;—the temperature at the solar surface is some 8,000° or some 10,000°, and the force of gravity there is some 27 times that on the earth. Laws of pressure and density found

under laboratory conditions may be greatly modified under conditions so dissimilar. At these temperatures elements are dissociated, and radiation, or light, pressure is a distinct factor: planetary gravitation may be at play, and the discrete particles of the corona may travel about the sun in independent orbits. It is clearly evident that the corona is not a true atmosphere, analogous to the atmosphere of the earth.

But the solar envelope does not end with the visible corona, it extends far out beyond the orbit of the earth. And this outer portion of the envelope, unlike the corona, can be seen by any one on any clear, moon-less evening of early spring. It is known as the Zodiacal Light and it appears as a faint, soft, lens-shaped beam of light, extending up from the horizon along the path of the sun. It is about as bright as the Milky Way, but it is not clearly defined, it fades away gradually into the general illumination of the sky.

The spectroscope shows that the light is reflected sunlight. It is reflected from bodies too minute to be seen individually, but sufficiently numerous to give the faint glow of the Zodiacal Light. These bodies, whatever their origin, form an immense group extending from the sun to far beyond the orbit of the earth; being very numerous in and near the plane of the ecliptic, densest and thickest near the sun, becoming fewer and more scattered as the distance from that body and from the ecliptic increases. These bodies are

apparently independent planetesimals, varying in size from the minutest dust particles to rocks and masses of iron of appreciable diameter; each revolving about the sun in its own independent orbit. Many of these paths intersect that of the earth, and collisions are frequent. When thus entrapped in the earth's atmosphere, these bodies become individually visible as shooting-stars and meteors. Most of the smaller particles are completely fused by the friction of the atmosphere, but some of the larger bodies reach the surface of the earth in the form of the well known meteorites.

These bodies enter the earth's atmosphere from every direction and with greatly varying velocities. While comparatively few reach the surface of the earth, the number of those consumed in the upper atmosphere is enormous. On any clear night half a dozen may be seen in the course of a few minutes, never an hour will elapse without one or two being visible. On some nights, at certain seasons of the year, the number that can be seen from a single spot can be counted by the hundreds. From many careful counts, made at different seasons and at widely different places, it has been estimated that from ten to twenty million meteors enter the earth's atmosphere each day. But the individual bodies are extremely small: from photometric observations, Newcomb showed that the majority of the meteors seen weigh less than a single grain. The largest and brightest observed by him in his series of deter-

minations, did not weigh more than a quarter of an ounce.

These minute bodies are consumed in the upper atmosphere, at heights ranging from 40 to 100 miles above the surface, and the products of their combustion gradually fall to the earth's surface in the form of very finely pulverized dust. Thus, the earth is gradually growing larger, but at an extremely slow rate. Even with the enormous number of meteors, as above estimated, the growth of the earth can hardly exceed a few tons per day, and it would take, at the present rate, many millions of years to increase the diameter of the earth by one inch.

Occasionally, three or four times a year, meteors are seen to fall to the surface of the earth, and many have been found and are now in museums and cabinets. One of the best collections of such meteorites may be seen at the American Museum of Natural History in New York City, which embodies many of the largest in the world. Here can be seen and handled the actual matter from outside space.

Space is not empty: and any deductions, any motions of the planets, obtained through the failure to take into account the mass of minute particles traversing space, are approximations.

CHAPTER IV

THE MOTIONS OF THE PLANETS

THE discussion of the motions of the planets about the sun may be greatly simplified by the use of the two fundamental approximations, discussed in the last chapter. The individual dimensions of the various bodies may be disregarded in the first instance, and the solar system may be considered, mathematically, as consisting of a small number of material particles, acted upon by their mutual attractions only, and situated and having their motions in empty space. In discussing the motions of such a system of imaginary mathematical bodies, Newton assumed, in addition to the law of gravitation, three fundamental laws of motion—three postulates, to use the word made prominent by the Relativity Theory. These laws of Newton, however, are simple and are based upon the experience of centuries. They are:

a. Every body continues in its state of rest, or of uniform motion in a straight line, unless it is compelled to change that state by forces impressed upon it.

b. The change in motion is proportional to the force impressed and takes place in the direction of the straight line in which the force acts.

c. To every action there is an equal and opposite reaction; or, the mutual actions of two bodies are always equal and oppositely directed.

The principles involved in these laws were known to Galileo, but to Newton is due the clear enunciation of them. The first law merely means that in empty space uniform rectilinear motion is just as natural as rest, that such motion implies no physical cause and requires no explanation. Change in motion, however, is different; change of any kind, in speed or in direction, implies force, external force, acting, or impressed, upon the body. This idea of motion and force is radically different from the conception of Aristotle and the older philosophers, who thought that whenever a body was in motion, some force must operate to keep it moving. On the contrary, no force whatever is required to keep a body in motion; force is required only to change the motion. If, through the action of some force, a body be set in motion and the force ceases then to act, the body will not stop when the force ceases, but will continue forever to move forward in a straight line at a uniform, or constant, speed.

Now, as change in motion, either in speed or in direction, implies the action of a force, so the amount of this change may be used as a measure of the size of the

force. If a certain force act upon a body, initially at rest, for one second, so that at the end of the second the body is moving with a speed of ten (10) feet per second; and if a second force act upon a similar body and impart to it a speed of twenty (20) feet per second; then is the second force twice as great as the first. The change in motion is proportional to the force impressed. It will be noted that the element of time also enters, for the longer a force acts upon a body, the greater its effect and the swifter will be the resultant motion of the body. All of this is shown in Figure 16, where A and B represent two cannon of different lengths and taking different powder charges, but firing

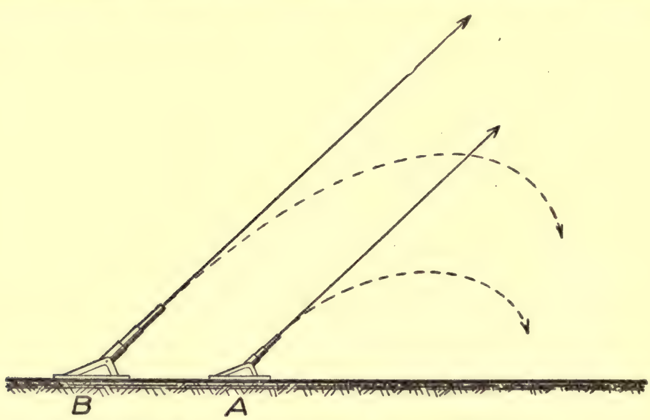


Fig. 16. Forces and Trajectories.

shells of exactly the same weight and size. The heavier powder charge in B produces a muzzle velocity, at the end of a definite fraction of a second, of exactly twice

that of A; hence the explosive force of B is twice that of A. On leaving the muzzles of the guns, the force of the explosion ceases to act upon the shells and, were it not for the friction of the air and the attraction of the earth, the shells would travel forever in straight paths and at constant speeds. But, no sooner have the projectiles left the muzzles than they are subject to two forces, one the resistance of the atmosphere and the other the attraction of the earth. The former acts directly in the line of motion and slows up the speed of the shells, but does not change the direction of motion; the attraction of the earth, however, pulls the shells downward and causes them to describe curved paths, finally bringing both to the ground. If the gun A were greatly lengthened so that the explosive force of the powder had a longer time interval to act upon the shell, then would its velocity be increased and the smaller force might thus impart the same velocity as the greater force in B.

Now suppose the air removed, so that there will be no friction, and the gun mounted on the top of a tower, so as to shoot projectiles horizontally at various speeds. Upon leaving the muzzle of the gun, the shell will be acted upon solely by the attraction of the earth. If the shell merely roll out of the muzzle, it will fall in a straight line to the surface of the earth; if the charge be sufficient to give the shell a small muzzle velocity, it will describe a curved path and fall to the surface

some miles from the foot of the tower. As the charge is increased, the muzzle velocity becomes greater and the range of the projectile longer. In every case, the explosive force of the charge ceases the instant the shell leaves the gun; from that moment, the only force acting upon the shell is the attraction of the earth, and it is this force of gravity that pulls the shell out of its straight line path and causes it to travel in the curved trajectory.

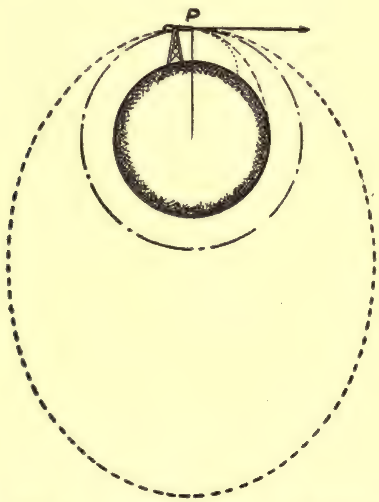


Fig. 17. Projectiles near the Earth.

The ordinary muzzle velocity of a shell is something less than half a mile per second and such a shell falls to the earth a few miles from the foot of the tower. If this velocity could be increased to something over four miles per second, the path of the bullet would be

nearly four thousand miles long, the bullet coming to the earth at this distance from the tower. If the muzzle velocity were increased beyond this point to about 4.9 miles per second, then the shell would miss the earth entirely and would travel about the earth in a circle. As the circumference of the earth is approximately 25,000 miles, it would take such a projectile travelling 4.9 miles each second about 5100 seconds, or 1 hour 24.7 minutes, to complete one circuit about the earth and to return to the starting point.

If the muzzle velocity were still further increased, the path of the projectile would become an ellipse and its period would become longer. When the velocity reached 6.94 miles per second, the path would become a parabola and the bullet would never return to the starting point.

These illustrations may help one to understand the motions of the planets. The force of gravitation, the attraction of the sun, keeps the planets in their respective orbits, but it does not explain or account for the original motion, for the muzzle velocity of the shell, so to speak. Once the planets were set in motion about the sun, gravitation took charge and regulated and controlled the various paths, but, somehow, in some inexplicable way the system was set in motion and each planet given an initial velocity. There have been many attempts to explain this initial velocity, to show how it may have been caused, but such attempts are specula-

tions, purely and simply. The law of gravitation deals with the facts of present day motions, it does not deal with the primal cause of motions.

Now consider a very simple, ideal case,—that of two perfect spheres of uniform density and separated by a measurable, finite distance. It has been seen that for spherical bodies the force of attraction varies only with the distance between their centres, and that, if the masses of the bodies be respectively M and m, and the distance between the centres r, then the mutual force of attraction is proportional to:

$$\frac{Mm}{r^2}$$

If these were the only bodies in the universe and if they were at absolute rest at the beginning of time, then this force of attraction would draw them towards each other, with different and rapidly increasing speeds. As the same force acts upon both bodies, the smaller will move the faster, and the bodies will finally come together at a point nearer the initial position of the larger. The point at which the centres would meet (disregarding the actual dimensions of the bodies) is what is commonly known as the centre of gravity of the system.

If, however, at the initial moment, the two bodies were in motion relative to each other and in any direc-

tion inclined to the line joining their respective centres, then, instead of falling directly toward the common centre of gravity, each ball would describe around such centre of gravity one of several simple geometrical curves. The centre of gravity would remain at rest, or would partake of any uniform rectilinear motion of the two bodies. The type and size of the curves described by the bodies depend solely upon the relative motion of the two bodies at the initial instant of time, just as the range of the gun depends upon the muzzle velocity of the projectile. These curves may be any of the curves known in geometry as conic sections, but in the vast majority of cases actually occurring in nature the curve is that known as an ellipse. If both bodies are of the same size, then the respective elliptical paths are also of the same size, but, if the balls differ in size, then the two orbits will also differ, the smaller body, however, will travel in the larger orbit. If the two bodies differ tremendously in size, as do the sun and the planets, then the common centre of gravity will be very near the centre of the larger body, may indeed lie within that body, and the smaller body will apparently describe an ellipse about the larger. In the case of the earth and the sun, the latter body is some 330,000 times as large as the earth, and, therefore, the radius of the sun's orbit is only some 280 miles as against the 93,000,000 miles of the earth's.

In the relatively crude observations at the time of

Kepler it could make little difference whether the earth was considered as traveling about the centre of the sun, or about the common centre of gravity of the two bodies. Not so, however, with the accuracy attainable with modern instruments. But it is a very simple proposition to find and to use the orbit of the earth *relative* to the sun's centre in place of the actual orbit about the centre of gravity. This relative orbit will be exactly similar to the actual one about the centre of gravity, except that it will be larger, will be magnified in the proportion of the total combined mass of the two bodies to that of the sun alone. That is, the relative orbit of the earth about the sun is to the actual orbit about the centre of gravity as $M + m$ is to M, or, in figures, as $330,000 + 1$ is to $330,000$. The relative orbit of the earth is thus about 280 miles larger than the actual one about the centre of gravity. And it is this relative orbit that is really used in all astronomical work, for the sun's centre is a distinct point, which can be accurately located in the heavens, while the position of the centre of gravity can only be determined by calculation. Further, the centre of the sun is a common point to which can be referred the motions of all the planets, instead of using for each planet the differently located centre of gravity of the system, sun and earth, sun and Jupiter, and so forth.

Thus, under the ideal conditions imposed, namely, that there are but two bodies in the universe and that

both of these bodies are spheres of uniform density, the path of the smaller body about the centre of the larger will be a conic section—an ellipse, a parabola, or an hyperbola. The character of the curved path and its size depend solely upon the initial velocity of the smaller body and its distance from the larger: the exact shape of the path and its position in space depend, not alone upon the velocity of the smaller, but also upon the direction of its motion. This is shown in Figure 18, where planets pass through P in various directions,

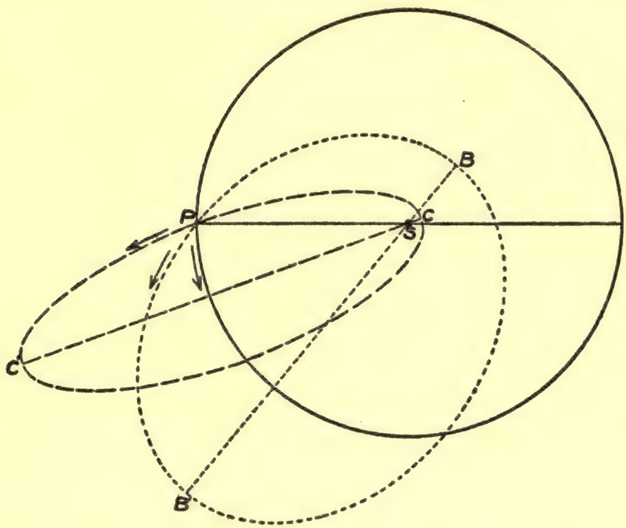


Fig. 18. Ellipses of the Same Size.

but always with the same speed. In every case the orbit is elliptical, the sun being at one focus, and the major axes of all the ellipses are all of the same length:

AA′ is equal to BB′ and to CC′: but the ellipticity of the various orbits and the positions of their respective perihelia are determined by the directions in which the planets pass through P.

If, therefore, the velocity of the planet at the point, P, is known, together with the direction in which it is moving, the complete path that it will describe can be found; and, when once the path is known, the position of the body at any time can be readily calculated. This is the celebrated Problem of Two Bodies, which was completely solved by Newton. The simple methods of ordinary mathematics, however, are not sufficient for this purpose, and Newton was obliged to devise special mathematical methods and formulas, which have since been developed into that powerful branch of mathematical analysis, known as Calculus. These methods are too intricate to be explained here, but they are well known to all mathematicians, and can be found in any ordinary text-book of mathematics.

The Problem of Two Bodies may be stated thus:

Given, at any moment of time, the positions, masses, and velocities of two spherical bodies, acted upon solely by their mutual attractions; required their positions and motions at any future time.

This is the problem that Newton solved. He showed that the two bodies would describe forever certain particular paths, or orbits as the astronomers call them; the exact shape of the path depending in each case upon

the motions of the bodies at the moment of starting. When the two bodies of the problem become, one the sun and the other any one of the planets, then Newton showed that the particular path of the planet would be an ellipse, as described by Kepler, and that the planet would move in such ellipse at the speed called for by Kepler's second law. Thus, in other words, Kepler's two laws of planetary motion are the direct and necessary result of the law of gravitation, and these two laws of motion are sufficient to predict the position of the planet at any future time.

Now in classifying the paths of the various planets and in locating their positions in space, astronomers make use of certain conventional terms, which are very simple and which ought to be fully understood. They call the quantities, which define the size, shape, and position of the path of a planet, the "elements of the orbit." These elements are six in number: two of them determine the plane in which the orbit lies; three of them, the shape, size, and position of the path in the plane of motion; and the sixth, the position of the planet in the curved path. The first two are the "inclination of the orbit" and the "longitude of the node," and their names clearly indicate their purely geometric character. In all astronomical work, the plane of the earth's orbit, the ecliptic, is taken as the fundamental plane of reference; and the inclination of an orbit is merely tne angle which the orbit makes with this funda-

mental plane of reference. These two planes intersect in a straight line passing through the sun, called the "line of nodes." In one part of this line, the planet, as it travels about the sun, will pass from the south to the north side of the ecliptic, and this point is called the ascending node. The direction in which this point lies is determined by its longitude, and this longitude is the element called "longitude of the node."

The three elements which determine the shape, size, and position of the orbit in the plane of motion are the "eccentricity," the "major axis," and the "longitude of the perihelion." The first two of these have the ordinary geometric meaning, while the third has been fully defined in a former chapter. It defines the direction in which the axis of the orbit lies in space. The five elements, so far defined, are clearly shown on the following diagram, which needs no explanation.

These six elements, which determine the motion of a planet about the sun, are tabulated as:

a = the semi major axis of the ellipse, or one-half the greatest diameter of the curved path.

e = the eccentricity of the orbit: a fraction from which can be determined the exact shape of the path. This becomes zero for a circular path, and unity for a parabolic orbit.

π = the longitude of the perihelion: an angle (or rather the sum of two angles) which locates the point of nearest approach of the planet to the sun.

☊ = the longitude of the node: an angle, which gives the direction of the point in which the planet passes from the south to the north

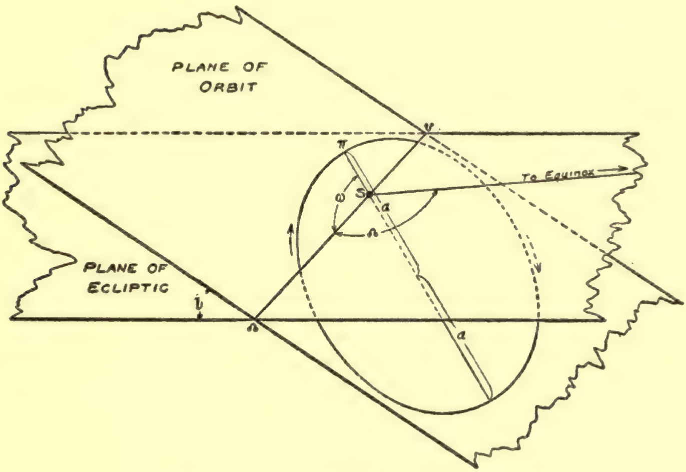


Fig. 19. The Elements of a Planet's Orbit.

side of the ecliptic, or plane of the earth's orbit.

i = the inclination of the orbit: the angle between the plane of the planet's orbit and that of the earth.

T = the time of perihelion passage: the exact hour, minute, and second at which the planet passes the perihelion.

The sixth element is required to determine the exact position of the planet in its path, for it is conceivable that there might be three or four separate bodies all

traveling about the sun in the same path, one behind the other. This element may be the longitude of the planet as seen from the sun at a given date, January 1, 1900, for example, or else the precise hour, minute, and second at which it passes through a definite point of the orbit, as the perihelion, or the node.

Now so long as there are but two bodies in the system, these six elements are constant, and the smaller body will travel for ever around and around in its unvarying path. From these elements the actual position of the body at any time, past, present, or future, can be calculated by very simple formulas.

If, however, a third body be introduced into our ideal universe, then the motions of the bodies are no longer simple and easily calculated. In fact, the paths of the three bodies become so complicated as to defy any mathematical description. Newton failed to find a solution of this problem; and every mathematician since his time has likewise failed. Yet
The Problem of Three Bodies is simple:

Given, at any moment of time, the positions, masses, and velocities of three spherical bodies; find their motions thereafter, and their positions at some definite future instant of time.

Acted upon solely by their mutual attractions, the paths that these bodies will describe are fixed, the positions that they will occupy at some future date are absolutely predetermined. Once the system is set in

motion, the future of each body is fixed, its path preordained. Thus the problem has a definite solution, but, unfortunately, no mathematician has yet been able to find that solution. The difficulty is entirely with the present day methods of mathematical research and calculation; these methods are inadequate. The beautiful method, devised by Newton to solve the problem of two bodies, fails completely, when applied to a system of three or more bodies. Under certain special conditions, mathematicians have been able to find an approximate solution of the problem, but even such approximate solution is extremely intricate. No solution of the general problem has been found.

This statement, found in all books, may need a little qualification, for it is possible, in very many cases, to trace out the paths of the bodies, step by step. To take a concrete illustration, the positions and velocities of the sun, the earth, and Jupiter are known for today. These velocities vary with the time, but for some very short interval of time they may be considered as constant. That is, for a single day, the motions of each of these bodies may be considered as being uniform and in straight lines. The amounts and directions of these motions depend, of course, upon the mutual attractions of the three bodies, and such amounts can be accurately calculated. Thus, from the measured, or known positions and velocities of the three bodies today, it is a matter of direct calculation to predict where they will

be tomorrow. Then from the positions and velocities of tomorrow, by the same process, can be calculated the positions for the next day. Thus step by step, it is possible to trace out the paths of the three bodies; the difficulty is in the shortness of the steps, and in the time required to make the calculations for each step. The calculations are long and it would take considerably more than a working day for a mathematician to trace out the path of the earth for twenty-four hours. Further, in many cases, the speed and direction of motion of a body change so rapidily, that a day even is too long an interval between steps. In tracing the motion of a certain comet, it became necessary to shorten the interval to fifteen minutes; to take ninety-six steps to trace its path for a single day. It required one month's time, six hours a day, to thus trace the path of this comet for one day. By still further reducing the length of the steps, practically every problem could be solved; but it might require years, or even centuries, to make the calculations necessary to trace the various paths of a system of bodies for a single week. The real problem is that of finding a mathematical short-cut; a short-cut, which will give accurate results and save this immense labor.

The special conditions, which allow of approximate solutions, are those found in the solar system—one great dominating central body accompanied by a number of relatively very small and distant bodies. In

this case the central body primarily controls the motions of its companions and holds them to approximate orbits. The interactions of the smaller bodies merely cause temporary irregularities in these orbital motions. This is shown in the following diagram, where S, the central body, is very large in comparison with the planets, E and J.

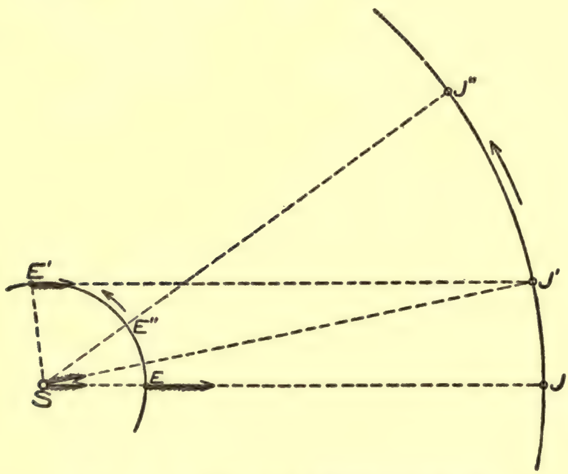


Fig. 20. Motions of a System of Bodies.

If the earth and the sun were the sole bodies in the universe, then the earth would travel about the sun in an elliptic path, as shown. But Jupiter attracts both the earth and the sun, and attracts these bodies differently. When the earth is directly between the sun and Jupiter, as at E in the diagram, Jupiter pulls both bodies directly towards itself, but as the earth is the nearer, the pull

upon the earth is the stronger. The respective distances of the earth and the sun from Jupiter are 4.2 and 5.2, and the force of attraction of Jupiter upon these two bodies varies inversely as the squares of these two numbers. The squares are respectively 17.6 and 27.0, which numbers are very closely in the ratio of 2 to 3. That is, the attraction of Jupiter on the earth is about 1½ times its attraction on the sun, as is indicated by the lengths of the herring-bone arrows in the diagram. But it is only the difference of these attractions that can affect the motion of the earth about the sun, and this difference amounts to only a small fraction of the whole attraction of Jupiter upon the earth. Further, the sun is approximately one thousand times larger than Jupiter and only one quarter the distance from the earth; so that the actual attraction of the sun upon the earth is some 16,000 times that of Jupiter. The differential attraction of Jupiter upon the earth, therefore, amounts to only about one fifty-thousandth part of the direct attraction of the sun. This minute effective pull of Jupiter in a direction contrary to that of the sun will, momentarily and to a very slight degree, straighten out the path of the earth: the curvature of the earth's orbit will be slightly diminished, and the orbit itself made a trifle larger.

The earth moves faster in its orbit than does Jupiter, and a few months later the three bodies will be in the respective positions, S, E′, and J′, at which time Jupiter

is equally distant from the earth and the sun. The attractive force of Jupiter upon the two bodies is now the same in amount, but it acts in different directions. Again only a small part of the actual force of Jupiter upon the earth is effective in disturbing the motion about the sun, and this disturbing force is only a very minute fraction of the direct attraction of the sun. In this position of the three bodies, the effect of Jupiter is to draw the earth and sun together, to make the earth's orbit a little more curved and slightly smaller than it otherwise would be.

As Jupiter and the earth move thus around the sun in their respective paths, they constantly change their relative positions, and, with the change in position, the disturbing effect of Jupiter's attraction upon the motion of the earth changes. At times it makes the orbit slightly larger, at times slightly smaller. But, after the lapse of some 399 days, the earth will again be directly between the sun and Jupiter; not, however, at E, but at E″ about 1/10th of the circumference in advance of E. At this point the action of Jupiter will be similar to what it was when the earth was at E: the curvature of the earth's orbit being diminished and the orbit itself made a trifle larger. If the orbits of both Jupiter and the earth were circles, this effect would be identical every time the planets came into line; it would be repeated over and over again every 399 days. But the orbits are not circular, they are elliptical, and so

when Jupiter returns to opposition, the relative distances between the planets and the sun will have changed, and the perturbation of Jupiter upon the earth's motion will not be the same as before. It will be the same in character, but not the same in amount. Thus the perturbation depends upon the part of the orbit in which the opposition happens; it will change in amount as the opposition falls in different seasons of the year. It is still periodic, it goes through regularly recurring changes, but the period is not the simple one of 399 days. The distance of the earth from the sun increases and diminishes again during one year, the distance of Jupiter from the sun fluctuates with its periodic time of nearly twelve years (11.86 years), and thus the actual value of the perturbation, or disturbance in the earth's motion, depends upon the 399-day period, the 365-day period, and the 11.86-year period, and upon other modifying periods too complicated to mention.

The attraction of the earth has a reciprocal effect upon the motion of Jupiter. When Jupiter pulls the earth outward and enlarges its orbit, the earth pulls Jupiter inwards and makes its orbit smaller; when Jupiter causes the earth to move faster in its path, the earth acts as a brake and slows down the speed of Jupiter. Thus the actual motions of the two bodies about the sun are extremely complicated; they move in wavy, snake-like curves, curves that are compounded

of all sorts of motions and periods. The planets do *not* travel in elliptic orbits and the laws of Kepler are *not* true.

From the time of Newton, it has been known that Kepler's laws are mere approximations, computer's fictions, handy mathematical devices for finding the approximate place of a planet in the heavens. They apply with greater accuracy to some planets than to others. Jupiter and Saturn show the greatest deviations from strictly elliptic motion. The latter body is often nearly a degree away from the place it would have been had its motion about the sun been strictly in accord with Kepler's laws. This is such a large discrepancy, that it can be detected by the unaided eye. The moon is approximately half a degree in diameter, so that the discrepancy in the motion of Saturn is about twice the apparent diameter of the moon. In a single year, during the course of one revolution about the sun, the earth may depart from the theoretical ellipse by an amount sufficient to appreciably change the apparent place of the sun in the heavens. This departure from strictly elliptic motion is not large enough, however, to be detected without the aid of instruments, but it is sufficiently large to be detected by the ordinary sextant of the navigator.

The real problem of the mathematical astronomer is to find some method, or methods, of approximately representing the actual paths of the planets; methods

by which the approximations may be made closer and closer, as the astronomer is willing to spend more and more time on his calculations. The scheme of elliptic orbits for the larger planets, of parabolic orbits for comets, furnishes the first step in the elaborate system of approximations that has been built up. The second step is involved in extremely complicated mathematics, but the underlying principle is not difficult to understand.

The mathematician considers the planet as always traveling about the sun in an elliptic orbit, but considers the orbit itself as constantly changing in size and shape. He thinks of the planet as a bead strung on a flexible wire, and this wire, which represents the orbit, as being pushed and pulled into various shapes by the action of the other planets; the bead always remains on the wire, but the wire is distorted into ellipses of various sizes and shapes. This may seem a rather round-about and complicated way of treating the motion of a planet, but, as a matter of fact, it is a labor-saving device; calculations that would be impossibly long, are by this method reduced to workable limits. It is another example of the old adage, that the longest way round is the quickest way home. The action of Jupiter, as heretofore illustrated, is directly upon the earth, not upon the orbit, for the orbit is purely an imaginary conception—a mathematical fiction. This action directly changes the speed of the earth and the direction in

which it is moving; and this change in speed and direction can be computed. With such corrected, or new, motion of the earth, a new orbit could be computed, which orbit would differ very slightly from the original. It might, for example, be slightly larger and with less curvature. Thus the direct effect of Jupiter is to change the actual motions of the earth; the indirect effect is to change the so-called orbit. Now, it is found to be easier and quicker to compute these indirect changes upon the imaginary orbit, and from these to find the actual changes in the earth's position, rather than to proceed in what might be called the normal straight forward way.

Another advantage of this indirect method becomes apparent after a moment's consideration. The action of Jupiter upon the earth is momentary, but the effect upon its motions is permanent. Suppose that the earth were the sole planet; its path would then be elliptic and constant—year after year, the earth would travel around in the same orbit. Now, if Jupiter were suddenly introduced into the system, as at J in Figure 20, it would pull the earth out of its path and enlarge its orbit. If Jupiter were then suddenly removed, blotted out of existence as it were, the earth would go on forever traveling in its new path. The earth could never return to its old orbit. Thus the effect of the momentary and slight disturbance in the earth's motion would be reflected down throughout all the ages. If

the temporary presence of Jupiter increased the length of the year by one second, then after the lapse of sixty years the earth would be one minute late in returning to E, an hour late after the lapse of 3600 years, and a whole day late after 86,400 years. The indirect change in the orbit thus affords a ready and easy means of finding the effect of the disturbance at some distant future date.

The astronomer, therefore, calculates the changes in the orbit of the planet, not the direct changes in the motion of the planet itself. The methods of computing these changes in the orbit of a planet are very complicated; the formulas would fill hundreds of pages of this book and would be incomprehensible to the ordinary reader, few physicists or mathematicians even have more than a dim idea of their real meaning, or the slightest knowledge of how to use them. These changes in the elements of a planet's orbit are called "perturbations" or "variations," and one may thus speak of the perturbation of the major axis of the earth's orbit, the variation of the perihelion of Mercury, or the perturbation of the eccentricity of Jupiter's orbit.

In calculating these perturbations, the mathematician is forced to adopt the old device of Hipparchus, the discredited and discarded epicycle. It is true that the name, epicycle, is no longer used, and that one may hunt in vain through astronomical text-books for the slightest hint of the present-day use of this device,

which in the popular mind is connected with absurd and fantastic theories. The physicist and the mathematician now speak of harmonic motion, of Fourier's series, of the development of a function into a series of sines and cosines. The name has been changed, but the essentials of the device remain. And the essential, the fundamental point of the device, under whatever name it may be concealed, is the representation of an irregular motion as the combination of a number of simple, uniform circular motions. It is so necessary that this device be fully understood, that, even at the risk of repetition, a fuller illustration is given in Figure 21.

Suppose C to be moving forward in the direction of the arrow at a uniform speed, while the particle A revolves in a circle about C, as a centre. As C advances, A also moves forward and downward, so that when C has moved to C′, A has made a quarter revolution and is directly in advance of C′ at D′. During this interval, the entire forward motion of the particle A has been equal to the sum of the forward motions of C and of A in the circle: the downward motion of A has been due solely to its motion in the circle. During the next quarter revolution, A will still be moving downward, but will be moving backward instead of forward, and its actual forward motion will be equal to the difference between the forward motion of C and the backward motion of A. At the completion

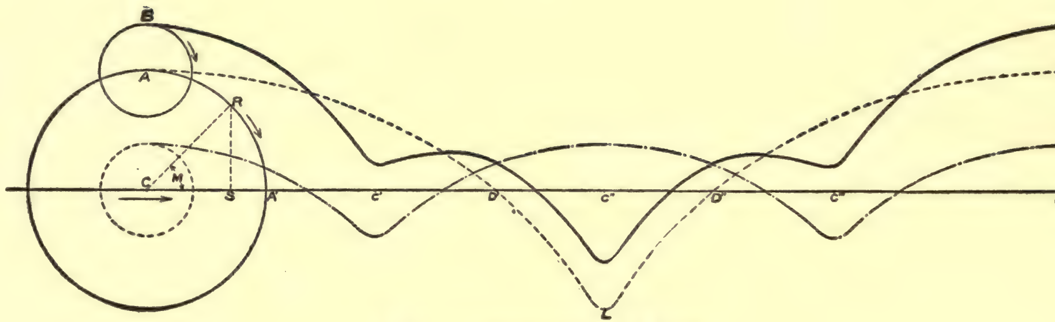


Fig. 21. Composition of Motions.

of one half revolution, A will be at L, directly under the then position of the centre, C″. The actual path of the particle is shown by the dotted line, AD′L, which is a combination of the straight line and the circular path.

Now, if another particle B be supposed to revolve in a much smaller circle about A, but at twice the speed; then, in a similar manner, this motion in the small circle can be combined with the forward motion of C, and the path represented by the dash-dot line would result. The actual path of B, however, would be the combination of this path with that of A, or would be as represented by the full heavy line. This actual path of the particle appears, at first glance, to be very irregular, but it is clearly made up of the combination of three very simple motions—one rectilinear, the other two circular.

It is not necessary to plot out the path on paper in order to find the real or combined motion of B. The methods and formulas of ordinary high-school trigonometry furnish a ready means. It will be remembered that for any angle, such as M in the diagram, the ratio of the base CS to the radius CR is called the cosine of the angle, and the ratio of the altitude SR to the radius is called the sine of the angle, and it will be further remembered that elaborate tables are at hand which give the numerical values of these ratios for angles of all sizes. Hence, if the length

of the radius CR be known, then for any particular value of the angle M,

$$CS = CR \; cosine\, M$$
$$SR = CR \quad sine\, M$$

and, by taking the values of the sine and cosine from the trigonometrical tables, these simple equations enable one to find the exact numerical values of CS and SR. But the particle A is supposed to move around the circle at uniform speed, therefore the angle M increases uniformly with the time. Suppose it takes just one hour for the particle to make a complete circuit, then in one minute it will travel 6°, or it will take 10 seconds for the particle to move over a single degree, and thus the exact value of the angle can be found for any given time. The length of time for the particle to travel once around the circle is called the "period," and the radius of the circle is called the "amplitude" of the motion.

The actual distance in the horizontal direction over which the particle has traveled at any given moment is equal to the distance through which C has moved plus the value of CS at that moment; the distance of the particle above the central line is equal to SR. Similarly for the motion of the particle in the smaller circle, the formulas of trigonometry give its position at any time. In the particular case illustrated in the

diagram it was supposed that B made just two revolutions while A made one. The angle which represents its motion, therefore, increases just twice as fast as that for A; or, if the one angle be M, the other will be 2M.

If the values of the radii of the two circles, or the amplitudes of the motions, be denoted for simplicity by a and b, respectively, and if c represents the rate at which the centre moves forward in unit time, one minute for example, then will the position of B, at any instant, be given by:

$$\text{Horizonal position} = c.t + a \textit{ cosine } M + b \textit{ cosine } 2M$$
$$\text{Vertical position} = a \textit{ sine } M + b \textit{ sine } 2M$$

From these two expressions any one, with the slightest knowledge of trigonometry, can find the exact position of B at any instant. But this position can also be found from the path as laid out in our diagram. The one method of finding the position of B requires the ability to use tables of trigonometry, the other method requires the ability of an expert draughtsman; the one method reduces the path of the particle to the form of a mathematical expression, the other visualizes it and traces it as it actually occurs in space. The former is the present method, known under various names; the latter is the old epicycle method of Hipparchus.

The graphical, or mechanical method can only be used when the motion is composed of very few and very large components. In simple cases, diagrams often suffice, and actual mechanical devices may be made to represent the motion. The Tide Predicting Machine of the Coast and Geodetic Survey at Washington is a note-worthy example of the application of the mechanical method. The rise and fall of the tide at any port is a periodic phenomenon, and it may, therefore, be analyzed, or separated into a number of simple harmonic, or circular components. Each component tide will be simple, will have a definite period and a constant amplitude; and each such component may be represented mechanically by the arm of a crank, the length of which represents the amplitude; each crank arm being, in fact, the radius of one of the circles in our diagram. Such a machine was invented by Sir William Thomson and was put in operation many years ago. The machine at present in use at Washington was designed by William Ferrel. It provides for nineteen components and directly gives the times and heights of high and low waters. In order to predict the tides for a given place and year, it is necessary to adjust the lengths of the crank arms, so that each shall be the same proportion of the known height of the corresponding partial tide, and to adjust the periods of their revolutions proportionally to the actual periods. Each arm must also be set at the

proper angle to represent the phase of the component at the beginning of the year. When all these adjustments have been made, the machine is started and it takes only a few hours to run off the tides for a year, or for several years. This machine probably represents the highest possible development of the graphical or mechanical method. It is a concrete, definite mechanical adaptation of the epicyclic theory of Hipparchus. But, because the Coast Survey represents and predicts the movements of tidal waters by a complicated mass of revolving cranks and moving chains, does any one imagine for a moment that the actual waters are made up of such a system of cranks? No more did Hipparchus believe that the bodies of the solar system were actually attached to the radial arms of his epicycles; his was a mere mathematical, or graphical device for representing irregular, complicated motions.

While the graphical, or mechanical method is limited to a few terms, the trigonometrical, or analytical method is unlimited. It is possible to pile epicycle upon epicycle, the number being limited only by the patience of the mathematician and computer. The expressions for the disturbing action of one planet upon another, due to the attraction of gravitation, involve an unlimited number of such terms; or, as the mathematician puts it, the series is infinite. The importance of each term depends primarily upon the size of the coefficient, or amplitude of that term—upon the length

of the crank-shaft, so to speak. Now these coefficients, the a's, the b's, and the c's, of the formulas, are themselves extremely difficult to calculate. Each one is made up of an unlimited number of separate terms, or is, itself, an infinite series. The principal factor in each coefficient is the ratio of the respective distances of the planets from the sun, and the size of the coefficient depends, therefore, primarily upon whether this factor, or its square, or its third power, enters. Neptune is thirty times as far from the sun as is the earth, and in the case of these two planets this factor is 1/30th. The square of this is 1/900, and the cube, or third power, 1/27000; so that the terms decrease very rapidly. In the case of Mercury and Venus, however, the ratio is about 1/2, and the powers of this factor are ¼, ⅛, 1/16, etc. These diminish much more slowly than do those for the Earth and Neptune, with the consequent result, that the computation is very much longer and more difficult.

The various angles, M, which represent the period of each term in the expression for the disturbed motion, are formed by various combinations of the mean motions of the planets. The earth, on the average, moves about one degree (1°) per day; Mercury four and one-tenth (4.1°) degrees. These may be combined in all sorts of ways: the direct difference between them is 3.1°; twice this difference is 6.2°; three times, 9.3°. Or again the motion of Mercury

less twice that of the earth is 2.1°, and twice this is 4.2°; or again the motion of Mercury less four times that of the earth is only one-tenth of a degree 0.1°. For each and every such combination, there will be a corresponding term in the disturbed motion of the planet. The period of the perturbation will be found by dividing 360° by the daily motion. For the relative motion of 3.1° per day the period is about 116 days, for the relative motion of 0.1° the period is nearly ten years.

The extreme complexity of the problem may be best illustrated by giving the actual expression for the position of the Perihelion of Mercury, as affected by the action of Venus alone. This is taken from the work of Leverrier, and in it the symbol λ represents the mean motion of Mercury and l′ that of Venus. The other symbols represent various elements, or combinations of elements of the two planets.

TABLE I

Perturbations of Mercury by Venus

$$
\begin{aligned}
{}^{*}\text{Perihelion of Mercury} = 75^\circ\, 7'\, 13''.93 \; & + 280''.6\, t \\
& - 0''.200 \sin\ (l'-\lambda) \\
& + 0''.205 \sin 2\ (l'-\lambda) \\
& + 0''.171 \sin 3\ (l'-\lambda) \\
& + 0''.117 \sin 4\ (l'-\lambda) \\
& \quad \text{etc.} \\
& \quad \text{etc.} \\
& \text{46 other terms}
\end{aligned}
$$

* Annales de l'Observatoire de Paris, *Mémoires*, vol. v.

Leverrier's calculations were made for January 1, 1850, and the time, t, is measured in centuries. So that, this expression means that on January 1, 1850, the perihelion of Mercury's orbit was located in longitude 75° 7′ 13″.93. Due to the action of Venus alone, the perihelion was then moving forward at a rate of 280″.6 per century. This uniform forward motion, however, is modified, also by Venus, by fifty periodic terms, which vary in size from 4″.58 to only 0″.015, and with corresponding periods of a few days to several years. In order to find the actual position of the perihelion on a given day, February 25, 1921, for example, the 71 years and 56 days, which elapsed between the date of Leverrier's epoch and the date in question, must be reduced to the equivalent fraction of a century (0.7115) and the 280″.6 multiplied by this figure. The result is 199″.65, or 3′ 19″.65. Then, from the known values of λ and l' must be calculated the exact value of each and every one of the fifty various angles: the sines of these must then be taken from a treatise on trigonometry, and each sine multiplied by the corresponding coefficient. For example, the first angle is 5.1°, the sine of which is + 0.089 Multiplying this by the coefficient, — 0″.200, the first term becomes on February 25th, — 0″.018. In the same way, all the other forty-nine terms are calculated; some are positive, some are negative. The algebraic sum of all these terms is + 7″.49, so that the position

of the perihelion on February 25, 1921, is given by:

$$\begin{array}{rrrl} \text{Perihelion} = 75^\circ & 7' & 13''.93 \\ + & 3 & 19\ .65 \\ + & & 7\ .49 \\ \hline 75^\circ & 10' & 41''.07^* \end{array}$$

It may be of interest to compare this position with those for other dates in the same year, remembering that the figures given were obtained by taking account of the action of Venus alone.

1921, February	25	75° 10′ 41″.07
July	19	33″.10
December	10	39″.45

These show that from February 25 to July 19 the perihelion was moving backward, while during the next period it was moving forward, but on December 10th it was still behind where it had been earlier in the year.

All this is complicated enough, but it only accounts for the action of Venus; it requires twenty-one (21) similar terms to account for the action of the earth, sixteen (16) for Jupiter, six (6) for Saturn, and one (1) for Uranus.

It has been noted above that each one of the coeffi-

* The effects of Precession and of the other planets are omitted: hence this value will not agree with that taken from the Nautical Almanac.

cients in the above expression for the position of the perihelion is itself the result of an elaborate calculation. That is, the figure 280″.6 for the secular motion due to Venus and the figure 0″.200 for the first periodic term, both depend upon, or are calculated from, infinite series, which series involve the ratios of the distances of Mercury and Venus from the sun, together with the eccentricities and the inclinations of their respective orbits. The necessary formulas for making these calculations are given by Leverrier in the Annals of the Paris Observatory, and it requires fifty-seven (57) quarto pages to print these formulas in a condensed and symbolical form. That is Leverrier uses the symbol, [1], for example, to represent a definite long series, which is printed once only, while the symbol, or abbreviation may enter the formulas half a dozen or more times.

The perturbations of the planets fall, as has been seen, into two great classes, the Periodic and the Secular.

The Periodic Perturbations are those which involve the trigonometrical sines and cosines. These depend upon the relative positions of the planets in their respective orbits; they are mostly small, and run through their courses in a few years, or a few revolutions of the planet in its orbit.

The Secular perturbations, however, are of special and extreme importance in all theories of

planetary motions. These are the terms that contain the time directly as a factor, similar to the 280″.6 in the expression for the perihelion of Mercury. The presence of these terms indicate permanent changes in the orbits, changes progressing steadily with the time; so that, after the lapse of centuries, the orbit will be completely changed in character. Advancing at the rate of 280″.6 per century the perihelion of Mercury would make one complete revolution in some 4600 centuries. Such a motion of the perihelion merely changes the position of the orbit in space; not so, however, with changes in the major axes, or in the eccentricities of the orbits. These elements determine the size and shape of the path of the planet, and any secular changes in these elements would mean the ultimate destruction of the solar system.

If the size of the earth's orbit were steadily decreasing, it would mean the ultimate collapse of the earth into the sun; if, on the other hand, it were found that the orbit were uniformly increasing in size, then would the earth be finally driven from the solar system and would disappear into endless space. Now some hundred years ago La Place showed clearly that this can never happen: there are no secular perturbations of the major axis of any orbit. In the mathematical expression for the length of the axis of an orbit there is no term corresponding to the 280″.6 in the motion of the perihelion. The distance of the planet from

the sun is, in the long run, constant; it suffers slight periodic variations, but always comes back to its average value after the lapse of a comparatively short period of time.

But the expression for the eccentricity of an orbit contains a secular term. This apparently indicates a permanent and progressive change in the shape of the orbit. The eccentricities of Mercury, Mars, and Jupiter are increasing, those of Venus, the earth, Saturn, and Uranus are decreasing. According to Newcomb * the secular changes in the eccentricities of the four inner planets are:

Mercury	+ 0.000,0206
Venus	— 0.000,0470
The Earth	— 0.000,0416
Mars	+ 0.000,0907

The change for Mars is the largest, and this figure would indicate that, after the lapse of some ten thousand centuries, the eccentricity of the orbit would be unity, and the orbit, itself, practically a parabola. Before this condition could be reached, however, the orbit would be so narrow that the planet would collide with the sun. Such secular changes in the eccentricities, if real, indicate, therefore, the final and complete collapse of the solar system.

* *Astronomical Constants,* page 109.

This question, as to the Permanency, or Stability of the Solar System, has been investigated by the leading mathematical astronomers; La Place, Leverrier, and Tisserand, to name a few of the most eminent. They have carried the system of approximations one step further, and have shown that in this next step the so-called secular perturbations of the eccentricities and inclinations, at least, disappear and are replaced by periodic terms of inconceivably long periods. These periods are measured by the thousands of years, and depend upon the relative positions of the orbits, not upon the positions of the planets in the orbits. The eccentricity of Mars, for example, instead of steadily increasing, will increase for centuries, then decrease until it becomes smaller than at present, then turn and again become larger. These fluctuations are confined within comparatively narrow limits; the orbits can never become circles, nor change radically from their present shapes. La Place clearly showed that the orbits of the various planets are so bound up together that, as the eccentricity of one orbit increases, that of some other orbit, or those of other orbits, must correspondingly decrease.

The so-called secular perturbations of the eccentricities and inclinations, certainly, do not exist; they are mathematical fictions, introduced through the special methods used in approximating towards the true values of these elements. The figures given for such secular

perturbations in all text-books, the figures found in the classic works of Leverrier and of Newcomb, are mere approximations to the present rate of change of the elements. The eccentricity of the earth's orbit is, at present, decreasing at the definite rate stated by Newcomb, but, after the lapse of centuries, this rate will change. For the next two or three hundred years, perhaps, there will be no appreciable change in this rate, and for all practical purposes of today it is sufficiently accurate to speak of this rate as constant, to speak of it as a secular perturbation. But, if one wishes to consider the condition of the solar system, the shapes of the planetary orbits, say ten or twenty thousand years ago, then these so-called secular perturbations must be discarded, and other and more accurate methods of calculation must be used.

But even these conclusions of La Place are only approximate; his methods are only a second or third step in the whole series of successive approximations towards the true motions of the various planets. Leverrier, as the result of an extensive research, concludes that it is impossible to determine whether the four inner planets, Mercury, Venus, the earth, and Mars, form a stable system or not. Tisserand has confirmed this result, and has specifically warned all students of Celestial Mechanics against the illusions of the stability of the planetary system.

It is thus seen that the mathematical astronomer is

forced to trace the motions of the planets by an elaborate system of successive approximations. The first step is comparatively simple; the second step is intricate and complicated to an almost impossible degree; further steps are impracticable to complete. These successive approximations or steps may be summarized as:

1st. Approximation:

The orbit of each planet is considered as being an unvarying ellipse, in which the body moves with a speed, which varies in a definite and easily computed manner. This speed is given by Kepler's second law.

This approximation is sufficient to give the positions of the planets, with one or two exceptions, for several years as close as can be observed with the unaided eye.

2nd. Approximation:

The orbit of each planet is still considered as elliptical, but the ellipse, itself, varies in size and position.

This approximation is sufficient for all practical purposes of the navigator and the routine astronomer, and gives the positions of the planets for a couple of hundred years, with the accuracy of minor telescopic observations.

This takes account of perturbations of the first order; considers the coefficients of all the terms

as actual constants, and the so-called secular perturbations as real and as progressing uniformly with the time.

3rd. Approximation:

Second order perturbations are included, and the secular perturbations are shown to be periodic in character, running their respective courses in immensely long periods of time.

This approximation carries the mathematical development to the highest degree possible, and indicates the modifications of the planetary tables necessary when very long periods of time are under consideration. It involves all discussions as to the permanency of the solar system, and as to its possible evolution.

CHAPTER V

THE MOTION OF THE PERIHELION OF MERCURY

WHILE MERCURY is a comparatively difficult object to observe with the unaided eye, it has been known from pre-historic times, and measurements of its position in the heavens run back as far as the second century before the Christian Era. The difficulty of observing the planet lies in its nearness to the sun.

Mercury, it will be recalled, is the nearest planet to the sun, and revolves about that central body in an elliptic orbit at an average distance of somewhat less than four-tenths (0.38) that of the earth. As it travels around the sun in this curve, the planet will, therefore, appear to an observer on the earth to oscillate backward and forward, appearing first to the eastward, then to the westward of the sun, but never departing very far from that body. When it is to the eastward of the sun, Mercury may be seen low down in the western sky for a short time after the sun has set: when it is to the westward of the sun, the planet rises before that body, and can be seen in the early hours of the morning. On account of the long twilights in northern latitudes, the planet is much more

difficult to observe in northern countries than in the lower latitudes of the Mediterranean, where the science of astronomy had its birth. Under the most favorable conditions, Mercury may be seen as far as 28° from the sun, and when in this position of greatest elongation the planet appears as a brilliant star of the first magnitude.

Most of the telescopic observations of Mercury are made in broad daylight, when it is invisible to the unaided eye. Such observations are not difficult to make, for the object-glass may be screened from the reflected glare of direct sunlight. And these daylight observations are preferable to those made after sunset, for the planet can be observed when high in the heavens and away from the mists and thick atmosphere that are always found near the horizon. But with all possible precautions, daylight observations are not as satisfactory as those made at night, for the sun heats the air, sets up currents, and produces all sorts of abnormal tremblings and refractions. The observations of Mercury, therefore, are not as accurate, not as satisfactory as those of Mars or of Jupiter, or of any other planet, which can be observed at night.

Due to the greatly varying distances between the earth and Mercury as they travel their respective paths about the sun, the apparent diameter of the latter planet varies from about 5″ to 13″. The real diameter of the planet is 3000 miles, or about 3/8ths that of the earth.

The path of the planet has caused considerable trouble to mathematical astronomers. This is largely due to the great eccentricity of its orbit and to its high inclination, but it has also been due, in the early days of astronomical research, to the difficulty of securing good observations. Kepler made tables of the planet's motions as early as 1627, from which he was enabled to predict the transit of the planet across the sun's disc in 1631. Halley in England about 1680 and Lalande in France about one hundred years later computed tables, which were far more exact than those of any preceding astronomer. These two sets of tables were of approximately equal accuracy, as was evidenced by the transit of 1786, for this phenomenon "took place three-quarters of an hour later than the time fixed for it by Lalande, and three-quarters of an hour earlier than that assigned by the tables of the English astronomer." Thereafter, Lalande greatly improved his tables: but it was Leverrier, who finally solved the difficulties and who in 1844 prepared tables of wonderful precision.

Leverrier based his researches upon a magnificent series of meridional observations of the planet made at the Royal Observatory in Paris. This series, as used by Leverrier, began on March 8, 1801, and extended to August 18, 1842, embraced somewhat over four hundred (400) separate observations in various parts of the planet's orbit, and covered one hundred and seventy-

two (172) successive complete revolutions of the planet in its orbit. He calculated, in the manner outlined in the last chapter, the periodic and secular perturbations caused by each of the six planets, Venus, the earth, Mars, Jupiter, Saturn, and Uranus. The development of the necessary formulas required mathematical ability of the highest order; the labor involved in the numerical calculations was enormous. Fortunately for Leverrier, Napoleon III was on the throne of France and ample funds were available for the corps of skilled computers necessary to carry out the immense task.

Some idea as to the intricacy of the problem and as to the labor involved may be gathered by again looking at the expression for the motion of the perihelion, as given on page 141, and remembering that this expression involves the action of Venus alone. To this must be added terms for the action of each one of the other planets, and when this is done, the work is only begun. For similar expressions must be found for the motion of each one of the other elements of Mercury's orbit, for the eccentricity, for the major axis, and the others. In all, Leverrier's formulas involved the use of 231 terms to express the periodic perturbations of the longitude and 118 terms for those of the radius vector. All these terms must be sorted out and collected into tables, so that the position of the planet on any given date can be determined. From such preliminary tables of motion, the position of the planet was determined

for the exact instant on which each one of the four hundred observations was made. These theoretical positions were then compared with the actual observed places of the planet, and, so accurate were these preliminary tables of Leverrier, that in all the forty-one years, the greatest deviation between the predicted and the true place (heliocentric longitude) was but $11''$.

This apparently marvelous agreement between theory and observation did not satisfy Leverrier, and he proceded to correct his tables, and to introduce, as further observations, the results of a considerable number of transits. These transits of the planet across the solar disc furnish the most delicate tests of any tables of motion, and locate, with extreme accuracy, the exact position of the planet at the moment of observation. To make the observation no delicate measuring instrument is necessary: a telescope large enough to see the planet and an accurate clock are all that are required.

At intervals of about 116 days, Mercury passes between the earth and the sun. If the orbits of the two bodies were in the same plane then, at these times of inferior conjunction, Mercury would appear to pass centrally across the disc of the sun. But the path of Mercury is inclined some $7°$ to the ecliptic and hence, in general, the planet appears to pass either to the north or to the south of the sun, as shown at A in figure 22. In this position the planet would not be visible, even with the aid of a powerful telescope, as

it would be concealed in the brilliant glare of the sun. But when the conjunction occurs near the node, N, in the diagram, then the planet passes across the face

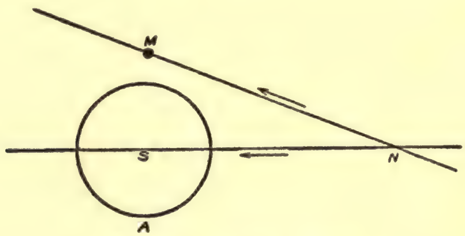


Fig. 22. Transit Limits.

of the sun, appearing as a small round, black spot. As the orbit of Mercury intersects that of the earth in two points in opposite parts of the heavens, there are two possible places where transits may be observed. The sun passes the ascending node of Mercury's orbit, as shown at N, on November 9th of each year, and the opposite, or descending node, on May 7th. Transits can only happen, therefore, when conjunctions occur on or near one of these days. The limit is about two days for May and about four days for November, so that November transits are nearly twice as numerous as those of May. During the last century there were four May transits and nine in November.

The observation consists in noting the exact instant at which the disc of the planet becomes tangent to the edge of the sun; and this instant can easily be determined to within a few seconds of time. There are

four such instants of contact, as will be noted from the accompanying figure: two external and two internal. The internal contacts can be observed with greater

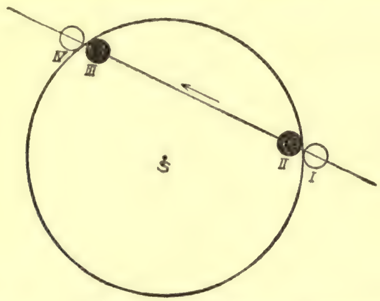


Fig. 23. External and Internal Contacts.

accuracy than the external ones. The first external contact is the most difficult, for it must be remembered that the planet is invisible as it approaches the sun, and does not become visible until it has advanced sufficiently far upon the solar disc as to make a decided black nick in the edge. There is some difficulty also with the internal contacts on account of irradiation and defects in telescopic definition. But with all these possible defects, a transit serves to determine the position of Mercury in its orbit with great accuracy. And such observations were of especial value before the year 1800, when the long series of meridian observations were begun in Paris. Before that date, observations of the planet, other than transits, were so crude as to be of little or no value to Leverrier in checking his tables.

The first transit ever observed was that predicted by Kepler, and occurred on November 7, 1631. This was observed at Paris by Gassendi, who in a letter gave the following interesting account: "The crafty god," says he, "had sought to deceive astronomers by passing over the sun a little earlier than was expected, and had drawn a veil of dark clouds over the earth in order to make his escape more effectual. But Apollo, acquainted with his knavish tricks from his infancy, would not allow him to pass altogether unnoticed. To be brief, I have been more fortunate than those hunters after Mercury, who have sought the cunning god in the sun. I found him out, and saw him, where no one else had hitherto seen him." This transit occurred 4h. 49m. 30s. in advance of the time predicted by Kepler. Unfortunately, the times given by Gassendi are not sufficiently accurate to allow Leverrier to make use of this observation in correcting his tables.

The first transit, which was observed with all the necessary accuracy, was that of November, 1697. This enabled Leverrier to check his tables with observations extending over a period of 145 years, or 601 complete revolutions of the planet in its orbit. In all he made use of nine (9) November transits and five (5) May transits; the last one to be included in his work was that of November 10, 1848.

After correcting his preliminary elements and tables of motion, Leverrier made a careful comparison of the

theoretical positions derived from his tables with the actual positions as given by the four hundred meridian observations and the fourteen transits. A very curious fact developed. There were no serious discrepancies between the theoretical and observed places so far as the four hundred meridian observations were concerned; nor were there any large errors furnished by the nine November transits. All these observations were completely satisfied and explained by Leverrier's theoretical orbit, as modified by the calculated perturbations caused by the other planets. Not so, however, with the *five May transits.* These five observations, between 1753 and 1845, showed discordances between theory and observation, which Leverrier deemed to be inadmissibly large. To quote his own words:

"Leaving aside the observations of 1661 *and* 1677, *it may be noted that, first the observations of the transits near the ascending node (November) give but small errors; while the transits near the descending node (May) give rise to an error of* 12″.05 *in* 1753, *which diminishing nearly regularly with the time, reduces to* – 1″.03 *in* 1845. *These thirteen seconds in* 92 *years require to be taken into serious consideration."*

"They cannot be attributed to errors of observation of the transits, for that is to suppose that all astronomers made considerable errors in the measures of the times of contact: these errors, moreover, must vary in a progressive manner with the time, and must differ by several minutes at the end of the period of 92 *years. Circumstances wholly inadmissible!"*

Now let us see just what these wholly inadmissible errors really are. In 92 years Mercury made 381 complete revolutions in its orbit about the sun and, during this time, travelled some trillions of miles, and, at the end of the period, was out of place, according to Leverrier's tables, by some 2300 miles, by about two-thirds (2/3) of its own diameter! If in addition to the real Mercury we conceive of an imaginary body of the same size as the real planet traveling about the sun in the theoretical path computed by Leverrier, then, during the 92 years, the real and the imaginary bodies would never have been completely separated. At the end of the period, the two bodies, the real planet and the imaginary one, would still be overlapping by one-third of their diameters. The disc of Mercury, itself, is too small ever to be seen by the unaided eye, and it would thus take several centuries for the distance between the real and imaginary planets to become great enough for the two bodies to be seen as separate and distinct. When the intricate formulas are considered and the enormous numerical calculations remembered, the wonder is, not that there should be this slight discrepancy in Leverrier's calculated places, but that the discordance should be so infinitely minute. The genius of Leverrier as a mathematician, and his patience and industry as a computer cannot be too highly eulogized.

In correcting his elements and adjusting his tables to conform to this minute discrepancy in motion, Lever-

rier was confronted by the difficulty already alluded to: the discrepancy appeared in observations made in only one part of the orbit. Observations made in all other parts of the planet's path were satisfactorily accounted for. Had this not been so, the discrepancy could very well have been accounted for by a minute correction in the mean motion of the planet, by changing the length of its year by a very small fraction of a second of time. Such a change, however, in the average speed of the planet about the sun would spread the discrepancy all around the orbit and it would appear proportionately in all the observations. The discordance, as found by Leverrier, could only be explained by some combination of motions, which counterbalanced, or neutralized each other in every part of the orbit, except near the descending node. The corrections to the elements, used in forming the tables, must be such that they nearly destroy each other for the November transits, while their effects are added together for the May transits. Leverrier found such a combination in changes to the secular perturbations of the *perihelion and of the eccentricity.* From the two sets of transits he deduced the relation between the necessary corrections to the annual motion of the perihelion, π', and that of the eccentricity, e'; namely

$$2.72\, e' + \pi' = 0''.392 \ *$$

* "Annales de l'Observatoire de Paris," *Mémoires,* vol. v, page 78.

Any values of π' and of e' that will satisfy this equation will also account for the discrepancies in the transit observations. A correction to the calculated value of the secular motion of the perihelion thus connotes a similar change or correction, in the computed secular term of the eccentricity. From the observations, alone, it is impossible to determine the individual values of e' and π': the numerical value of the total combination only can be fixed.

Now, according to Leverrier's calculations, the entire secular motion of the perihelion amounts to 526″.7 in a century, or to 5″.267 per annum, while the corresponding secular changes in the eccentricity are 4″.2 per century, or 0″.042 per annum. Any large change in the term of the eccentricity is precluded by the very size of the motion to be corrected and by other considerations. Leverrier finally concluded that by far the greater portion of the necessary correction must be made in the motion of the perihelion. A reason for this is easily seen, for if the computed secular motion of the perihelion (5″.267) and of the eccentricity (0″.042) be combined in a manner similar to the equation above given, then the numerical value of such combination would be 5″.381. But the correction to this was found to be 0″.392, or a trifle over 7 per cent. And the corresponding percentage of the whole motion of the perihelion is about 0″.38 per annum: this is the value of the correction finally adopted by Leverrier.

There will, of course, be a corresponding correction to the eccentricity, which can be found from the equation, and which amounts to about 10 per cent of the original computed value. To again quote from Leverrier:

"The necessity for a considerable increment to the secular motion of the perihelion results exclusively from the observations of the transit of the planet across the disc of the sun: further only times of internal contacts, which can be observed with great accuracy, were used. To escape from this necessity, it must be admitted that errors of several minutes in the estimation of the time of this phase must have been made in the large observatories, for example in 1743 *or in* 1753 *at Paris, and by observers such as La Caille, de Lisle, Bouguer, the Cassinis.—An unacceptable hypothesis!"*

A possible explanation of the discrepancy might lie in the use of erroneous masses for the planets in all the computations upon which the perturbations depend. The mass of a planet, which has a satellite, as Jupiter for example, can be very accurately determined; but the determination of the mass of a planet, without a satellite, is one of the most difficult problems of celestial mechanics. The masses of Mercury and Venus can only be found through the effects of their attractions upon other bodies; that of Venus, through the perturbations caused in the motions of Mercury. The mass of Venus is a direct factor in the computed value of the annual motion of the perihelion of Mercury, and

this mass is uncertain. In his computations Leverrier used the value 1/401,800 for the mass of Venus [that of the sun being unity], and with this value he found that Venus caused 280″.6 out of the total motion of the perihelion of 526″.7 per century. To be the cause of the 38″ excess motion, the mass of Venus would have to be increased in the proportion of 38″ to 281″, or by practically one-seventh (1/7). Of course this increase in the mass of Venus, necessary to account for the excess motion of the perihelion, would increase in like proportion all the perturbations of the other elements of Mercury and those of the other planets as well. And such an increase would cause many discrepancies: discordances far worse than that in the motion of the perihelion. It thus appears very clearly that the perihelial motion cannot be due to the use of an erroneous value of the mass of Venus, and its cause must be sought elsewhere.

Leverrier found the most plausible explanation of the observed excess in the motion of the perihelion to be the presence of a planet, or a group of planets, between Mercury and the sun. He announced the results of his calculations to the Academy of Sciences on September 12, 1859, and showed that a body, about the size of Mercury itself and revolving about the sun at a distance about midway between that luminary and Mercury, would so act upon Mercury as to fully account for the unexplained motion of the perihelion. If

the body were nearer the sun, its mass would have to be greater; if nearer Mercury, its mass would be less. While such a body could never be seen under ordinary circumstances, it ought to be conspicuous at the times of total solar eclipses, and it might be seen in transit across the solar disc. Leverrier discussed the possibility of such a body being observed, and called attention to the fact that a ring of asteroids would produce the same effect upon the motion of Mercury and might easily escape detection in the glare of the sun.

Following almost immediately upon the publication of this paper came the announcement that such a planet had actually been observed in transit across the sun's disc on March 26, 1859, by Dr. Lescarbault. Leverrier investigated this reputed discovery, and was convinced of its substantial accuracy. He named the new planet "Vulcan," and computed elements of its orbit. Several similar observations were afterwards reported, and the planet was even detected on the Greenwich photographs of the sun. From the elements of the orbit Leverrier predicted the dates of future transits. On the days set, however, no object was seen against the solar disc, which could, by any possibility, be a planet in transit. Much doubt was thrown upon the original observation of Dr. Lescarbault, and several of the other reputed planets in transit were shown to have been sun-spots.

Fresh interest was aroused in "Vulcan," when two

well known American astronomers rediscovered it during the total solar eclipse of 1878. Watson and Swift each claimed to have seen in the vicinity of the obscured sun, not one, but two planets. Yet, neither one of the four supposed planets could be identified with Leverrier's "Vulcan," which, if real, must have been at that moment on the other side of the sun. Astronomers finally came to the conclusion that both Swift and Watson had been mistaken in their observations, and had really seen two fixed stars, not planets.

But for several eclipses the search for intra-Mercurial planets was one of the principal objects of every expedition. During the few moments of totality, leading astronomers carefully examined the vicinity of the sun for suspicious objects; photography was brought into service. No planet has been found, and belief in Vulcan has ceased to exist. All the evidence to-day would seem to show conclusively that there is not, within the orbit of Mercury, a body sufficiently large to be detected, nor large enough to account for the peculiar motions of Mercury.

While it is thus now known that "Vulcan" does not exist, yet the second suggestion, that of a group or ring of planetoids near the sun, finds more and more confirmation. This suggestion was deemed by Leverrier, himself, to be extremely probable, but it was lost sight of in the excitement caused by the reputed discoveries of a single planet.

Some twenty years after the classic papers of Leverrier were published, Newcomb, in Washington, undertook an extension and revision of Leverrier's work. Four transits had occurred in the meantime—three in November and one in May—and it appeared of extreme importance to include these observations in the test. The entire work was based upon that of Leverrier, whose intricate calculations of the motions and perturbations of Mercury were taken as correct: correct theoretically and numerically. Before this assumption was made, however, one portion of the work was checked. George W. Hill, the eminent mathematician of the American Ephemeris and Nautical Almanac, recomputed the secular perturbations of Mercury caused by Venus, and used formulas and methods of computation quite different from those of Leverrier. Hill's results agreed remarkably with those of Leverrier: the centennial variation of the perihelion being according to Hill 280″.50 instead of the 280″.64 found by Leverrier. The difference between these results is considerably less than one-tenth of one per cent (0.05%) of the computed value, and is less than 1/270th of the 38″ discrepancy. It would thus seem to be clearly established that Leverrier's classic work is substantially correct, both theoretically and numerically.

Newcomb* thoroughly investigated each and every

* *Astronomical Papers of the American Ephemeris and Nautical Alanac.* Vol. i: 1882.

one of the transits observed between the years 1631 and 1881. He collected all the recorded observations, making use of many that had been overlooked or discarded by Leverrier. It will be remembered that Leverrier took account of the internal contacts only; Newcomb, on the other hand, utilized all four contacts, wherever possible, in each observed transit. In some instances, of course, one or more of the contacts were wholly unobserved, due to clouds in the sky, or to the sun being below the horizon at the moment. A number of the individual observations were rejected as being inconsistent with the majority made at the time; and some observations, when made under obviously poor conditions, were given small weight. Newcomb painstakingly investigated each and every dubious observation and tried to obtain, from the many observations made at each transit, the time which most correctly represents the actual occurrence. Some idea of the difficulties encountered may be had by reference to the transit of 1753. Seventeen observations of external contact are cited by Newcomb; the difference between the earliest and the latest time, as given by the individual observers was 67 seconds. Eleven of these observations were discarded by Newcomb as being either clearly erroneous or of dubious value. The six remaining observations differed among themselves by 22 seconds. Newcomb took the mean of these six as representing the real time at which the phenomenon

took place. Similarly in the transit of 1799, the individual recorded times of external contact differed by 1 minute and 41 seconds. After rejecting several of these as probably worthless, there remained twelve observations, which differed among themselves by 58 seconds. The mean of these twelve was taken by Newcomb as the observed time of contact, and this mean was found by him to differ from the theoretical or calculated time by only 34 seconds. Some of the actual observations, thus, differed by only three or four seconds from the time computed theoretically from Leverrier's tables.

To show how difficult it is to determine the actual time at which a contact actually took place, let us compare the times of internal contact in the transit of 1845, as determined by Newcomb and Leverrier respectively. The latter astronomer used ten observations, made by the best observers in Europe, which differed among themselves by eleven seconds. Newcomb used twenty-five observations, several of which were made in America. Two of those used by Leverrier he discarded, so that his twenty-five observations consisted of eight used by Leverrier and seventeen that were apparently not available at the time the French astronomer completed his work. Each astronomer took the mean of the observations used; Leverrier of his ten, and Newcomb of his twenty-five. The results differed by six (6) seconds. In other words, disregarding all

theoretical considerations, all hypothetical orbits and formulas of celestial mechanics, and considering solely the observations of reputable astronomers, Newcomb fixes the time of contact in the transit of 1845 six seconds earlier than Leverrier. This may seem to be an almost negligible quantity, but the entire discrepancy between theory and observation, at that moment, amounted to only nineteen (19) seconds. The difference between Newcomb and Leverrier as to the actual observed time of the phenomenon amounts, thus, to nearly one-third (⅓) of the entire discordance to be explained.

However much the two researches may differ in detail, the general results are in full and complete accord. Newcomb found that the discordance between the observed and theoretical motions of the perihelion of Mercury, as pointed out by Leverrier, really exists and is, in fact, slightly larger than he supposed. Newcomb gives this excess motion as 43″ per century, as against the 38″ found by the eminent French astronomer.

There is one element of uncertainty in all these conclusions, and that element is the mass of Venus. The size of the perturbations of Mercury depend upon this mass of Venus, and the mass, itself, can only be found from the perturbations. There is here somewhat of a vicious circle. But, the mass can be found, not only from the motion of the perihelion, but also from the motion of the node and from a number of the periodic

inequalities. Newcomb investigated this question and determined the mass of Venus in a number of different ways, using, of course, in each case the motions as actually observed. Some of these results are shown in the following table:

From the perihelion of Mercury	$\frac{1}{347,800}$
From the node of Mercury	$\frac{1}{408,400}$
From the periodic inequalities	$\frac{1}{396,000}$

the mass of the sun being unity. Every method of determining the mass of Venus, except that of the motion of the perihelion, leads, thus, to a figure approximating closely to 1/400,000, and this exceptional figure differs by more than ten (10%) per cent from all the others. There is a decided preponderance of evidence in favor of the figure 1/400,000. Further, if the mass of Venus were taken as being 1/347,800 so as to fully account for the motion of the perihelion, large discrepancies would be at once introduced into the motion of the node and into many of the larger periodic perturbations. The accumulation of evidence, to-day, points rather to a smaller mass for Venus; the accepted value being 1/408,000, or practically the same as that derived from the motion of the node.

Thus it follows, that, with the best attainable values of the masses of the planets, the calculated motion of the perihelion of Mercury is less than the observed value, and this discrepancy must be considered as real.

Newcomb continued his researches on the motions of the planets, redetermining, from all available material, the elements, masses, and motions of the four inner planets. He used Leverrier's tables as the basis of his work, and determined the corrections to Leverrier's elements, which would best satisfy all the conditions furnished by the observations. The entire work was inter-locking and the labor involved enormous. The number of meridian observations of the various planets actually included in the investigation was:

Of the Sun	40,176
Mercury	5,421
Venus	12,319
Mars	4,114
Total	62,030

In addition there were included all the transits of Mercury and of Venus, involving many more hundreds of separate observations.

As the numerical values of the perturbations depend directly upon the masses of the planets, Newcomb made a new and exhaustive investigation and redetermination of the masses of all the principal planets. That of Jupiter was found from the motions of its

satellites, from its action upon Saturn, upon two of the planetoids, and upon a couple of comets; that of Mars from the motions of its satellites; and that of the earth from the many and varied measures of the solar parallax. The masses of these three planets were thus obtained from independent sources and are known with a relatively high degree of accuracy. In determining the masses of Mercury and Venus, Newcomb used the periodic perturbations solely, so that the mass of Venus is derived from the periodic perturbations of Mercury and of the earth, produced by its action; the mass of Mercury is derived from the periodic irregularities in the motions of Venus. Thus the masses of all the planets, as used by Newcomb, are entirely independent of the secular motions of the elements; independent of the motion of the perihelion of Mercury.

As this classic and monumental research neared its completion, Newcomb found secular variations of the elements, besides that of the perihelion of Mercury, which could not be satisfactorily represented by the equations and formulas of celestial mechanics. In a note, published in the Astronomical Journal, October, 1894, he called specific attention to irregularities in the motion of the node of Venus, and in the motions of the node and eccentricity of Mercury. The final results were published in the form of a Supplement to the American Ephemeris and Nautical Almanac for 1897, which was printed in Washington in 1895. Discrepan-

cies between the calculated and observed values of the secular variations were found to exist for nearly every element of the four planets; four of these discrepancies were so large that Newcomb called especial attention to them. These four, as enumerated by Newcomb, are:

1. The motion of the perihelion of Mercury	$+\ 41''.6$
2. The motion of the node of Venus	$+\ 10''.2$
3. The perihelion of Mars	$+\ 8''.1$
4. The eccentricity of Mercury	$-\ 0''.88$

In this final table of results, the value of the discordance for the perihelion of Mercury is reduced somewhat from that given by Newcomb in his preliminary work, heretofore mentioned. The final and definitive value is:

$$+41''.6$$

per century. In the same table, the discordance in the eccentricity of Mercury is given as:

$$-0''.88$$

In his entire investigation there is no mention of the peculiar connection between the variations of the perihelion and the eccentricity, so clearly brought out by Leverrier; at least, Newcomb does not directly use any such equation as that deduced by Leverrier from a consideration of the May and November transits. Yet the above results of Newcomb, derived independently, almost exactly satisfy the Leverrier equation:

$$2.72\ e' + \pi' = 0''.392$$

Thus the two investigations clearly indicate that the motion of Mercury cannot be explained by a correction to the perihelion alone; there must be a corresponding correction to the eccentricity. Owing to the greater magnitude of the perihelial discrepancy, it has been stressed in all discussions and has been given a prominent place in all text-books, while the corresponding, but equally important, discrepancy in the eccentricity has been completely lost sight of, and no mention of it is to be found in any text-book or popular treatise on the motions of the planets.

While these two magnificent investigations of Leverrier and Newcomb place the existence of the discrepancy in the motion of the perihelion beyond all doubt, they do not definitely fix its numerical value. The result of Leverrier differs from the preliminary result of Newcomb by 5″, and is 3″.6 smaller than Newcomb's final definitive value. The exact numerical value, assigned to this discrepancy, is a compromise, an adjustment of many individually varying observations. The individual observations of the transits vary among themselves, and it is impossible to fix, beyond all doubt, the exact instant at which any particular phase of a transit occurred. Newcomb and Leverrier differ by six seconds in their adopted times for a single phase of the transit of 1845. Newcomb assigns a probable error of $\pm$ 1″.4 to his final result; a mathematical way of stating that the chances are even that the actual

error in his determination is greater or less than this amount. Taking into consideration the intricacies of the problem, the discordances among the individual observations of the recorded transits, and the intimate connection between the motions of the perihelion and the eccentricity, it is perfectly clear that the actual value of the discordance in the motion of the perihelion may differ by some seconds from that given by Newcomb. The mean between the values as found by Leverrier and Newcomb is 39″.8, practically 40″. And this figure probably represents, as nearly as it is humanly possible to determine, the actual discrepancy in the motion of the perihelion.

Since the "Vulcan" fiasco there have been many attempts to find an explanation of the excess motion of the perihelion; to find a physical cause for this motion. Newcomb, in the paper above mentioned, made a detailed examination of all the suggestions, and clearly showed that the difficulty is not how to account for the motion of the perihelion, but how to account for that motion without introducing other complications in the motions of Mercury and of the other planets. He showed that the motion of the perihelion can be fully accounted for by any one of several possible distributions of matter in the immediate vicinity of the sun and the inner plants. He, however, discarded each possible explanation because of the difficulties encountered in explaining, at the same time, the

motions of the other planets. Each possible explanation of the motion of the perihelion introduced a new complication somewhere else in the solar system.

The simplest explanation is to be found in the first fundamental approximation of celestial mechanics. It will be remembered that in all the formulas of motion, in all the numerical calculations of the perturbations, the sun and all the planets are assumed to be spheres of uniform density. This assumption is obviously not true for the planets, and is probably not true for the sun. A simple calculation will show that a very small departure from sphericity in the sun will suffice to account for the motion of the perihelion of Mercury. In fact, if the equatorial diameter of the sun should be only 0″.6 greater than the polar, then, this ellipticity alone is sufficient to give the perihelion of Mercury a motion of 40″ per century, and to fully reconcile the difference between the observed and the calculated motions. Newcomb, however, pointed out two difficulties in the way of accepting this explanation; the heliometer measures of the sun made by the German observers, and the fact that any such ellipticity of the sun would introduce other discordances into the system.

An ellipticity of the sun sufficient to produce the observed motion in the perihelion of Mercury would also produce various motions in the other elements. It would give rise to a forward motion in the node of Venus and thus go far towards accounting for the

second large discrepancy pointed out by Newcomb. But it would also cause a retrograde motion of 2″.6 in the inclination of Mercury's orbit. The motion of the inclination, as calculated, is smaller than the observed motion by some 0″.38, and the ellipticity of the sun makes this discrepancy worse, not better; makes it, in fact, practically 3″.0. The whole secular motion of the inclination, as observed, is only 7″.14, so that a discordance of 3″.0 is 42% of the whole; and any such disagreement between theory and observation is, of course, wholly out of the question. For this reason, Newcomb justly concludes that the motion of the perihelion cannot be explained in this simple manner.

On the other hand, it is practically certain, as was pointed out in a former chapter, that the sun is slightly elliptical, and such ellipticity must produce some motion in the perihelion. This ellipticity is probably not over 0″.10, but is almost certainly at least one-half of this. For the sun is a rotating mass of gas, and the normal shape of such a rotating body is that of an oblate spheroid. From the known dimensions of the sun and its period of rotation it can readily be shown that, due to its rotation, the equatorial diameter should exceed the polar by about 0″.05. But an ellipticity of even this amount would cause a motion of about 3″.5 per century in the perihelion of Mercury; an ellipticity of 0″.10, the amount indicated by the meas-

urements of the solar disc, would give rise to a motion of $7''.0$ per century.

Some portion of the observed discrepancy is thus undoubtedly due to the shape of the sun; but this portion is relatively small and cannot amount to more than one-sixth of the total necessary for a complete explanation.

Another explanation of the discrepancies between theory and observation is to be found in the second fundamental approximation of celestial mechanics,—the assumption of empty space. It is known that this assumption is not true, that, on the contrary, there are vast quantities of bodies occupying the spaces between the planets. Leverrier, who first called attention to the discordances, believed that a full explanation of them could be found in either a single planet, or in a group of planetoids in the space between Mercury and the sun. Newcomb investigated the probable action of groups of planetoids, or rings of matter, lying in various positions between the sun and Mercury, and between Mercury and Venus, and of an extended mass of diffused matter similar to that which causes the zodiacal light. Any one of these hypotheses can be used to fully explain the discordance in the motion of Mercury's perihelion, but each and every one is discarded by Newcomb on account of other difficulties encountered. According to him, the hypothesis which best represents all the conflicting motions of Mercury

and Venus is that of a ring of planetoids between the orbits of the two planets. In this case the difficulty encountered is the large inclination of the ring, necessary to account for the motion of the node: Newcomb finds this inclination to be $7^\circ.5$, slightly greater, therefore, than the inclination of Mercury's orbit. Newcomb deems such a great inclination highly improbable, believing that there would be a tendency for the planes of the orbits of such a ring of planetoids to scatter themselves midway between the planes of the larger planets and the invariable plane of the solar system. Regarding the possibility of orbits of the necessary great inclinations, Newcomb says,

"In admitting such orbits we encounter difficulties which, if not absolutely insurmountable, yet tell against the probability of the hypothesis."

Yet another objection raised by Newcomb is in connection with the discordance in the motion of the perihelion of Mars, which is entirely similar to that of Mercury and amounts to $8''.1$ per century. Those distributions of matter, which might account for the motions of Mercury and Venus, account for only a very small portion of this excess motion of Mars; leaving this to be accounted for in other ways. Reasoning by analogy, if the motion of Mercury is due to a ring of undiscovered matter, then that of Mars ought most certainly to be due to the known ring of planetoids, which revolve within the orbit of Jupiter. Sev-

eral hundreds of these bodies are known, the largest being about 485 miles in diameter, and the smallest probably not over 10 miles. The total mass of the known bodies is altogether too small to have any appreciable effect upon the motions of Mars. From the number and magnitudes of those already known, Newcomb tried to estimate the probable mass of the entire group. Admitting that the zodiacal light and the "gegenschein" are probably due to light reflected from these bodies, too minute to be seen separately, Newcomb concludes that the total mass is far too small to produce the observed effect. He regards as "unsatisfactory" the hypothesis that the motion of Mars is due to the presence of these bodies.

Newcomb's conclusions in regard to the existence of a group of planetoids, sufficiently large to account for the observed discordances, is expressed as follows:

"It seems to me that the introduction of the action of such a group into astronomical tables would not be justifiable. The more I have reflected upon the subject the more strongly seems to me the evidence that no such group can exist, that whatever anomalies exist can not be due to the action of unknown masses of matter.

"Besides, the six elements of such a group would constitute a complication in the tabular theory."

Newcomb was confronted by the necessity of having to prepare planetary tables for the use of the American Ephemeris and Nautical Almanac; tables which would

best represent the actual motions of the planets. In order to prepare such tables, a reconciliation of some sort, between theory and observation, had to be adopted. He found that practically all the discrepancies, except those of the various perihelia, could be reasonably well accounted for by small corrections to the assigned masses and elements of the planets, and that the discordances in the motions of the perihelia of Mercury and of Mars were fairly well represented by Hall's hypothesis regarding the law of gravitation. This hypothesis was suggested by Hall in 1894, and is that the gravitation of the sun is not exactly as the inverse square, but that the exponent is a fraction greater than 2 by a certain minute constant. This, of course, is a direct modification of Newton's law.

Under Newton's law of gravitation, the force between the two bodies of masses m and m′, and at a distance apart equal to r, is given by:

$$\text{Force} = \frac{m\,m'}{r^2}$$

Under the law suggested by Hall, the exponent 2 would be increased by a very small fraction, so that the force between the two bodies would be:

$$\text{Force} = \frac{m\,m'}{r^{2+\delta}}$$

When δ is a very minute fraction, this law results in giving the perihelion of the planet a small forward

rotation, without affecting any other element. In this regard, therefore, this Hall hypothesis is strikingly similar to the Einstein law, and antedates this latter by many years. Hall found that when δ, in this formula, was made equal to

$$0.000\,000\,16$$

then the forward motion of the perihelion of Mercury would be 43″ per century, and the discrepancy would be fully accounted for.

Such a modification of the law of gravitation would, of course affect the perihelia of all the other planets, and Newcomb computed these as:

For Mercury	+	43″.37
Venus	+	16 .98
Earth	+	10 .45
Mars	+	5 .55

These quantities, for Mercury and for Mars, agree very well with the values of the observed discrepancies; but the agreement fails utterly in the cases of Venus and the earth.

Now what Newcomb actually did in forming his tables was to compromise; to accept no theory as to the actual cause, or causes, of the observed irregularities, but to adopt corrections to the elements and masses in such forms that they can be readily distinguished from the values derived from purely theoretical grounds. To quote:

"What I finally decided on doing was to increase the theoretical motion of each perihelion by the same fraction of the mean motion, a course which will represent the observations without committing us to any hypothesis as to the cause of the excess of motion, though it accords with the result of HALL'S hypothesis of the law of gravitation; to reject entirely the hypothesis of the action of unknown masses, and to adopt for the elements what we might call compromise values between those reached by the preceding adjustment and those which would exist if there is abnormal action." *

* *The Elements of the Four Inner Planets and the Fundamental Constants of Astronomy.* Washington, 1895.

CHAPTER VI

THE MOTIONS OF THE PLANETS AND THE RELATIVITY THEORY

THE EXCESS MOTION of the perihelion of Mercury furnishes, according to Einstein, a complete confirmation of the Relativity Theory. In attempting to establish his theories by this excess motion, Einstein constantly refers to it as something unique, as the sole irregularity in the solar system. This idea that Mercury is the one planet to exhibit an irregularity of motion, is the sole exception in an otherwise orderly solar system, is expressed by Einstein in clear, unequivocal language. There can be no misunderstanding of phrases such as the following:

"The sole exception is Mercury, the planet which lies nearest the sun."

"That for all the planets, with the exception of Mercury, this rotation is too small to be detected with the delicacy of the observation possible at the present time."

The PREFACE to the book "Relativity," in which these statements appear, was written in December, 1916: the third edition was published in 1920. And

this definite position of the author of Relativity in regard to the motion of Mercury is still more clearly established by a letter written by him on July 30, 1921 —a letter written for publication and which has been printed. In this letter appears the clear cut declaration:

"The perihelial movement of Mercury is the only anomalous one in our planetary system which has been sufficiently attested."

Yet Leverrier in 1859, in the very paper in which he announced the discovery of this "perihelial movement," showed that its very existence connotes a similar irregularity in the eccentricity; Newcomb in 1894 specifically called attention to discordances in the motions of other planets, and, in his final, definitive work published in 1895, he gives the amounts of *eleven* discordances and calls particular and especial attention to four of these. Thus, according to Newcomb, there are, at least, four anomalous motions in the planetary system, which have been fully attested; and his statement is supported by a classic research involving over 60,000 observations and by a life-time devoted to the study of similar questions. Against these statements of Leverrier and Newcomb, statements backed up by years of patient astronomical research, Einstein sets his mere opinion, the opinion of one, who, however eminent he may be as a mathematician, is not an astronomer, and has never made, so far as known, an

astronomical investigation of this character. Can it be that the author of Relativity is unaware of these statements of Newcomb? Can it be possible that he has never read the very papers, upon which the astronomical proof of the Relativity Theory is supposed to be based?

The Relativity Theory can explain a certain definite amount of motion in the perihelion of Mercury. According to the formulas and computations of Einstein this amounts to 43″ per century; it can account for neither more nor less. There is no flexibility in the Einstein formulas, no constant of uncertain value, no possibility of adjustment. The difference between the actual motion of the perihelion and the theoretical motion under the Newtonian law of gravitation must be *exactly* this amount, if the Einstein theories are true: no other amount will satisfy the principles of relativity. The actual motion can be determined by observation only; the theoretical motion by calculation alone; and it is certainly remarkable that the difference between observation and theory should be exactly the 43″ required by the relativity theory. This is a most striking coincidence, and this coincidence has been stressed as proof positive and conclusive of the Einstein doctrines. Yet this coincidence of figures is largely due to the astuteness of Einstein in quoting the result of Newcomb's preliminary investigation, and in ignoring the classic work of Leverrier and the final

results of Newcomb. According to Einstein the results of the astronomical investigations into the motions of Mercury are summed up as:

"it was found (Leverrier—1859—*and Newcomb*—1895) *that an unexplained perihelial movement of the orbit of Mercury remained over, the amount of which does not differ sensibly from the above mentioned* + 43 *seconds of arc per century. The uncertainty of the empirical result amounts to a few seconds only"* (152).

Leverrier in 1859 found 38″: Newcomb in 1895 found 41″.6; quantities quite different from the 43″ quoted by Einstein. This latter figure was the result of Newcomb's first investigation, published in 1882; it does not appear in Newcomb's final work as published in 1895. In one portion of this classic investigation Newcomb makes use of a "provisional value of 40″.7." His definitive value is found only in the tables, where it appears as multiplied by the eccentricity. He gives for this product the value + 8″.48; from which by simple division it is easy to derive the specific amount + 41″.6, as the excess motion of the perihelion. The mean of the two results—Leverrier 1859 and Newcomb 1895—is 39″.8, which differs by 3″.2, by 8 per cent, from the figure quoted by Einstein. The coincidence of figures, the supposed agreement between observation and the relativity theory, vanishes the moment the real facts are stated.

Further, as has been pointed out, the sun is a rotating

body; and from its known size, density, and period of revolution can be computed its theoretical shape as an oblate spheroid. And such computations rest upon physical laws and can be made with the same theoretical accuracy that Einstein uses in finding the "relativity motion" of Mercury. Such a theoretical oblateness of the sun would cause a motion of at least $3''.5$ per century in Mercury's perihelion. The actual measurements of the sun indicate a somewhat greater oblateness, and hence a larger motion in the perihelion. But, using the theoretical value and deducting it from the observed value of $39''.8$, there is left over an unexplained motion of the perihelion of Mercury of not over $36''.3$. Thus the real amount of the unexplained perihelial motion of Mercury differs by $6''.7$ from the $43''$ of the Einstein theory, differs by 16 per cent of the required amount. And such a difference is very nearly fatal to the Relativity Theory, for that theory contains no arbitrary constant by which Einstein can, in the future, readjust his figures to fit the real and not the imaginary facts.

The relativity theory, thus, does not satisfactorily explain even the one discordance in planetary motions, which has been so thoroughly exploited by Einstein and his followers. If his methods and formulas fail to meet the test in this one selected case, selected by the author of relativity, how do these same formulas and methods fare in tests with other observed planetary

discordances? For, Einstein to the contrary notwithstanding, there are many anomalous movements in the planetary system; anomalies, which should be explained and accounted for by any hypothesis or theory which pretends to remake the universe. In his discussion of the elements of the four inner planets Newcomb found eleven anomalies among the fifteen secular motions, and of these eleven he singled out four as requiring especial consideration.

Now, the formulas and theories of Einstein differ from the formulas of the Newtonian mechanics in one point and one point only. Under the relativity formulas the perihelion of a planet will have a forward rotary motion; but every other element and motion of the planet will be the same as that under the Newtonian law. Mathematical discussions of the Einstein theory, too formidable to enter into, show that, *"The only secular perturbation is a motion of the perihelion."** Thus the relativity theory cannot explain, or account for, any of the observed discrepancies in the motions of the planets, other than those in the perihelia.

But it is clear that, under the relativity theory, the perihelia of all the planets must rotate by various amounts depending upon their respective distances from the sun. The amounts of such rotations can be

* *On Einstein's Theory of Gravitation, and its Astronomical Consequences,* by W. de Sitter.
Monthly Notices, R.A.S., vol. lxxvi, No. 9, page 726.

readily calculated from the formula given by Einstein for the case of Mercury. This Einstein rotation is independent of the mutual action of the planets upon one another and may be directly compared with the discordances as determined by Newcomb. The results of such comparison are shown in the following table:

TABLE II

Observed Discordances and the Einstein Motions

	Tabular Discordances	Per cent	Einstein motion	Final Difference
PERIHELIA:				
(1) Mercury	$+41''.6 \pm 1''.4$	7.2%	$+42''.9$	$-1''.3$
Venus	$-7''.3 \pm 22''.3$	17.2%	$+8''.6$	$-15''.9$
Earth	$+5''.9 \pm 5''.6$	0.5%	$+3''.8$	$+2''.1$
(3) Mars	$+8''.1 \pm 2''.6$	0.5%	$+1''.3$	$+6''.8$
NODES:				
Mercury	$+5''.1 \pm 2''.8$	0.7%	0	$+5''.1$
(2) Venus	$+10''.2 \pm 2''.0$	0.6%	0	$+10''.2$
ECCENTRICITY:				
(4) Mercury	$-0''.88 \pm 0''.50$	26.5%	0	$-0''.88$

The first column, in this table, gives the values of the discordances between theory and observation as determined by Newcomb: the large bracketed numbers give the order in which Newcomb arranged the four especially large discrepancies. Each one of these discordances is followed by the so-called "probable error" of the determination. These probable errors give some idea as to the relative accuracy of the various deter-

minations, but it must be remembered that the assignment of these probable errors is very largely a matter of judgment, and that these values may be over- or under-estimated. In every step of the long and complicated computations an estimate, rather than an exact calculation, has to be made as to the value of the probable error, and the final value, as given in the table, thus depends upon many separate estimations or judgments.

The second column gives the percentage that the discordance bears to the observed motion. While this is rather an unusual way of comparing results, it may be of interest and it may throw some light upon the problem. It will be noted that three of the discordances represent very large percentages of the observed quantities; and of these three, two are among the discordances specifically singled out by Newcomb. Of the remaining two, that of the perihelion of Venus is peculiar. In the case of this planet the computed value of the motion of the perihelion is greater than the observed motion, thus giving the discordance the negative sign. Again this discordance is over 17 per cent of the total observed motion, yet Newcomb estimates it as being only about one-third of the probable error. It would seem that where the percentage is so large, the discordance must be real and that the size of the probable error has been over-estimated.

The third column gives the Einstein motion, as com-

puted from his formulas, and the final column, the discrepancies between theory and observation after allowing the full and complete Einstein effect.

An inspection of this table shows that the Einstein motion is sufficient to account for practically all the "tabular" discrepancy in the motion of the perihelion of Mercury, and to reduce, in a marked manner, that for the earth. It accounts, however, for only a very small portion of the discordance in the case of Mars, and more than doubles the already large discrepancy in the case of Venus. It does not account for the motion of Mars nearly so well as does the Hall hypothesis. And further, the Einstein law does not in any way account for the important discrepancies in the motions of the nodes and in the eccentricity of Mercury.

Einstein, himself, disregards the motions of all the planets except Mercury, dismissing from consideration their perihelial movements with the simple statement, *"for the other planets of our solar system its magnitude should be so small that it would necessarily escape detection."* This statement may be perfectly true for some of the theoretical Einstein motions, which in the case of Mars amounts to only 1″.3, but it certainly does not apply to the observed motions of the perihelia, and it conveys a distinctly erroneous impression. Twenty-five years before Einstein wrote these words, Newcomb detected and measured such movements, found that the perihelion of Mars rotates by an un-

explained 8″.1 per century, by an amount six times the whole Einstein effect. It is hard to see upon what scientific grounds it is allowable to select one result of a scientfic research and to dismiss all the others as negligible, why one figure is to be taken as absolutely accurate and all other figures thrown out as worthless! It certainly cannot be a question of mere size, that 43″ is accurately measurable, and that 8″ is too small even to be detected. In many an astronomical research 8″ is a quantity of supreme importance. The fundamental unit of astronomy, the solar parallax, is only 8″.8; the parallaxes of the fixed stars are measured in minute fractions of a single second.

The motion of the perihelion of Venus is peculiarly embarrassing for the relativity theory. According to Newcomb's results, the perihelion of this planet is rotating more slowly than the computations indicate it should, the difference being 7″.3 per century. The Einstein formulas would increase the theoretical speed of rotation by an additional 8″.6, thus making the total discrepancy between observation and theory 15″.9, or 37 per cent of the entire observed motion! The Einstein formulas, in this case, make a bad matter worse; they give the orbit a rotation in the opposite direction to that which is required to fit the observations. It is perfectly true that the eccentricity of the orbit is small and that, on this account the Newcomb determination of the motion is liable to a much larger error than

in the cases of other planets. But it is hardly likely that any such error would be large enough to change the direction of motion.

Thus the Relativity Theory is not *sufficient* to explain the discordances in the planetary motions. It accounts *approximately* for only one among the numerous discrepancies—that of the perihelion of Mercury. It fails completely to explain any portion of several well-attested irregularities—those of the nodes and eccentricities; and it doubles the observed discrepancy in the motion of Venus. A simple investigation will show that the theory is *not necessary* to explain even the one discordance which it can more or less account for:—the motion of the perihelion of Mercury can be fully accounted for by the action, under the Newtonian law, of matter known to be in the immediate vicinity of the sun and the planets.

Newcomb, many years ago, showed that this motion can be completely accounted for by any one of several possible distributions of matter in or near the sun. The difficulty which faced Newcomb and the astronomers of the Newtonian school, is not how to account for the motion of Mercury, but how to account for it in such a way as to explain, at the same time, the other observed irregularities. This difficulty, which appeared nearly unsurmountable to Newcomb, is readily disposed of by Einstein by the simple expedient of saying that such other irregularities do not exist, or rather

have not "been sufficiently attested." If this "relativity" method of surmounting the difficulty be adopted by the astronomer, then is he faced by an embarrassment of riches and the problem is reduced to one of mere choice, to the selection of that solution which best pleases one's fancy. For the motion of the perihelion of Mercury, taken by itself, can be explained equally well by:

1. A non-spherical sun.
2. A ring of matter between Mercury and the sun.
3. A group of planetoids outside the orbit of Mercury.
4. The Hall hypothesis.

And there are good reasons for accepting each one of the three first mentioned: the sun is non-spherical, and matter is known to exist both within and without the orbit of Mercury.

But, if the methods of the author of relativity are to be admitted, there is no necessity of explaining the perihelial motion of Mercury. If it is troublesome to our theories, it can be discarded along with all the other discordances. Why even bother about Mercury itself! Copernicus is said never to have seen the planet; and the solar system would really be much simpler without it!

CHAPTER VII

THE ECLIPSE PLATES AND THE RELATIVITY THEORY

THE SECOND ASTRONOMICAL proof of the Relativity Theory, cited by Einstein, is apparently supported by very much stronger evidence. This proof, as shown in Chapter II, rests upon the deflection of light observed by the British astronomers at the total solar eclipse of May 29, 1919. As presented in the "Report," the evidence makes a strong prima facie case for the Relativity Theory. It was this evidence and the way in which it was presented to the Royal Astronomical Society at the meeting of November 6, 1919, that caused the furor in regard to the Einstein theory and its acceptance by so many scientists. But on an examination it will be seen that the strength of this evidence has been greatly magnified, and that many elements, which tend to weaken its force, have been omitted from the public announcements and from popular, or semi-scientific expositions.

The eclipse expedition of 1919 was organized by the leading scientists of England, and was participated in by astronomers, trained through long years of scientific research. The final report, giving the results

of the expedition, was drawn up by Sir F. W. Dyson, F.R.S., Astronomer Royal, Prof. A. S. Eddington, F.R.S., and Mr. C. Davidson. This report is a scientific paper of the highest possible value, and will, undoubtedly, rank among the classic papers of astronomy. In it are to be found the scientific details of the expedition, presented in a clear and well-ordered manner. But, as the report was prepared by eminent astronomers, accustomed to the intricacies of astronomical methods, many of the essential details are embodied in long and complicated looking tables, which, while perfectly clear to the reader trained in these methods, are meaningless to the average scientist, be he a mathematician or a physicist. Few, if any, scientists, other than a small group of astronomers, have probably ever read anything more than the concluding paragraphs of the Report.

In the following pages an attempt is made to present the essential elements of the Report and the results of the expedition in untechnical language.

The expedition was divided into two parties; one of which, under the direction of Professor Eddington, went to the island of Principe, off the west coast of Africa; and the second party, under the direction of Dr. Crommelin, to Sobral, a small town some miles inland from the north-east coast of Brazil. The two parties were equipped with horizontal photographic telescopes; the object glasses of which were borrowed

from Oxford and Greenwich respectively. These lenses, known as astrographic, were made especially for photographic work and are 13 inches in diameter. The horizontal type of mounting, used by the expedition, is very convenient for field work, and has been almost exclusively used in eclipse expeditions but it involves some disadvantages for accurate measurements, as will be seen by reference to the following diagram.

The telescope, itself, is mounted horizontally in a fixed position, the tube resting upon temporary piers. In front of the telescope lens, B, is mounted a plane mirror, A, which is driven by clock-work; such a moving mirror being known as a cœlostat. The mirror of such instruments are made of heavy glass and are coated with a thin film of silver on the front surface; the light being reflected from the silver face and not entering the glass itself. A ray of light from the eclipsed sun is reflected from the mirror into the lens of the horizontal telescope, where it passes to the photographic plate at C. Now, as the sun rises in the heavens the mirror must be rotated, as otherwise the reflected beam would soon pass off the lens of the telescope, and such rotation is provided by the clock-work. As the mirror is generally in an inclined position, its diameter must be greater than that of the lens, otherwise the beam of reflected light would be too narrow to cover the entire surface of the lens.

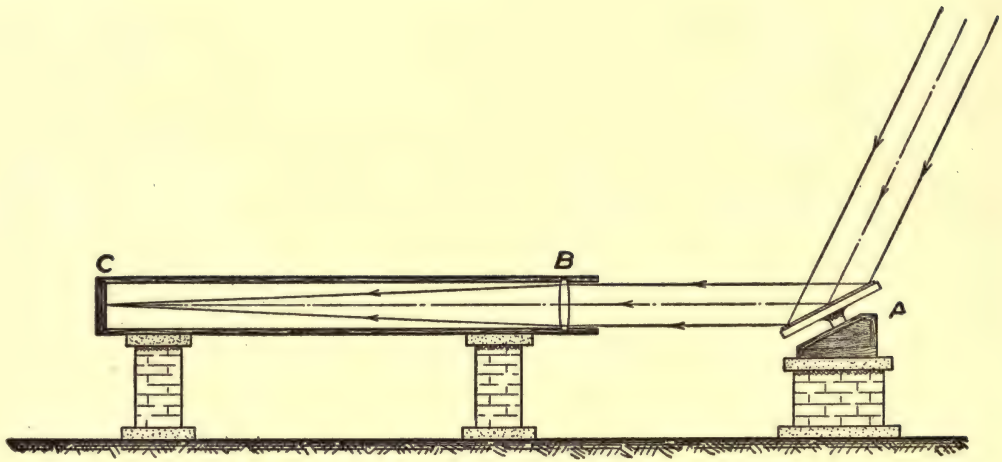


Fig. 24. A Horizontal Telescope.

The mirrors, used by the expedition, were 16 inches in diameter.

It will be at once apparent that such a horizontal arrangement is much more convenient than the ordinary type of telescope mounting. In the latter the tube of the telescope, BC, is pointed directly at the sun, or other heavenly body, the mirror being, of course, dispensed with. But the tube, in this case, must be hung on axes, so as to follow the movements of the body as it travels across the heavens. Such a direct telescope mounting, especially for a large lens, is an elaborate piece of mechanism, involving very large and heavy parts. And such mounting must be constructed for the locality in which it is to be used; so that the heavy, permanent mounting constructed for Oxford would be of no use in Brazil, or on the island of Principe. It would be practically impossible to modify such a mounting and to transport it to some out of the way district, and there erect it for temporary use. On the other hand the astronomers of the Lick Observatory of California have evolved a form of temporary mounting suitable for field work in which the mirror is discarded and the lens is always pointed directly at the eclipsed sun.

For many purposes the horizontal telescope is perfectly satisfactory: for obtaining photographs of the corona, for spectroscopic investigations, the introduction of the mirror is no disadvantage. But for

exact, minute measurements the mirror introduces complications. In the first place the instrument is no longer symmetrical; horizontal and vertical distances in the heavens are reflected differently by the mirror, and this asymmetry will always introduce an element of doubt as to the value of results. Again, a mirror is far more delicate than a lens: a slight distortion in the shape of a mirror will produce approximately three times the effect upon the image that a similar distortion in a lens will produce. Very slight changes in temperature will produce marked distortions in the shape of a mirror; the mere tilting of a mirror from one position to another, causing its own weight to rest more on one portion than another, will often so change the shape of a mirror as to cause noticeable distortion in the image. These facts have been known for many years, certainly ever since the transit of Venus in 1882, when the results obtained with horizontal telescopes were not satisfactory.

In addition to the regular astrographic object glass, the Sobral party took, as an auxiliary, a 4-inch telescope of 19-feet focus, loaned by the Royal Irish Academy. This instrument had been used in conjunction with an 8-inch mirror by Father Cortie in Sweden in 1914. This lens was mounted in a square wooden tube, and the mounting of the mirror was adjusted for the difference of latitude between England and Brazil by bolting it upon a strong wooden wedge.

On a photographic plate taken with this instrument a second of arc (1″) is represented by a distance of approximately one nine-hundredth (1/900th) of an inch.

Now before discussing in detail the actual results obtained by the two eclipse parties, it will be well to understand something of the methods and the difficulties involved. A photograph of a portion of the heavens shows the various stars as minute dots, the images of the brighter stars being larger than those of their fainter companions. The principal stars of the eclipse field and their relative positions are shown in

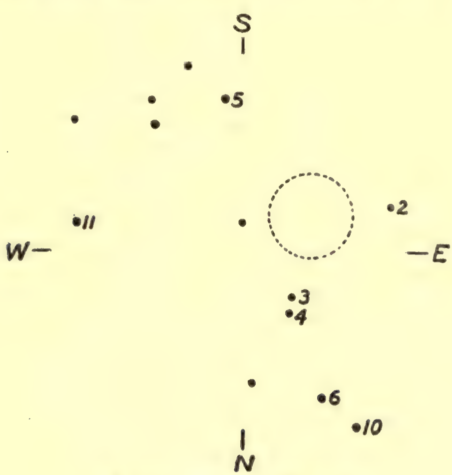


Fig. 25. The Eclipse Field.

the accompanying diagram, in which the dotted circle represents the eclipsed sun. The numbers, near each star, are those used in the Report for identification.

The unnumbered stars did not show on the photographic plates, or not on a sufficient number of plates to be of any use.

The original photographic plate, the negative itself, is put in a measuring machine, under microscopes, and the relative positions of the images carefully determined. The measurements are made with a carefully calibrated micrometer screw, and are given in terms of revolutions of the screw. These screw revolutions must be turned into seconds of arc by determining the scale value for the particular plate or plates being measured. This scale value depends upon the distance of the plate from the optical centre of the lens, upon the focal length of the lens in other words. The focus of a lens does not remain absolutely constant, especially when the lens is used under widely varying conditions of climate and temperature. Therefore the scale of photographs taken with the same lens at different times and places may vary. Again it is impossible to exactly orient a plate and this introduces another difficulty. But standard methods of measurement and reduction have been worked out by which these and other instrumental difficulties are overcome, and the results freed from their effects.

Now the distance between the various stars on the eclipse plates are large and the direct measurement of these distances would be difficult, so an indirect method was adopted. A special photograph of the region

was taken with the plate reversed, so that the light passed through the glass plate before reaching the film. When this plate was developed, it could be placed in contact with one of the regular eclipse plates, film to film, and the images of the same star on the two plates would appear very close together. The small distances between the corresponding images of the two plates could be measured with extreme accuracy. This special, or scale plate, merely provided a convenient point of reference near each star, to which could be referred the images on each and all the plates in turn. While this method gives with extreme accuracy the shift of the images from plate to plate, it does not, of course, give the actual coördinates of the various stars. But it gives accurately and quickly everything that is necessary for the investigaion of possible light deflections.

There is in the whole method, however, an inherent astronomical difficulty, a difficulty that is really serious. The light from the stars passes through the atmosphere of the earth, and is refracted, or bent out of its course. Each star appears higher in the heavens than it really is, and the amount of this refractive effect depends upon the height of the star above the horizon, upon the temperature of the air, and upon the barometric pressure. At any given instant, such as that at which a photograph is taken, the various stars on the plate will be at different heights above

the horizon, and will, therefore, be refracted differently. It is this "differential refraction" as it is called, that is of importance, for in the present investigation one is interested solely in the relative positions of the various stars, and not at all in the actual height of the group, as a whole, above the horizon. The amount of this differential refraction is calculated by theoretical formulas, derived for standard conditions of the atmosphere, with corrections for temperature and pressure. The entire amount of the refraction for the stars on the eclipse plates was approximately one hundred times the sought-for shift, and the differential refraction between the various stars amounted to several times the full expected light deflections.

Further the actual amount of the refraction at any time depends largely upon the rate of decrease of temperature of the air with the height above the surface of the earth, and this rate may be quite different in day-time from night-time. For the sun heats the surface of the earth, and the air near the surface is, therefore, warmer in day-time than at night. As most astronomical observations are made at night, the tables of refraction represent night observations and night conditions of the atmosphere, rather than day observations and day conditions. For this reason, there is a certain element of uncertainty introduced, when the eclipse plates are corrected for the effects of refraction by the use of tables primarily calculated

for and based upon night observations. Again, cases of abnormal refraction are not infrequent; instances where local currents of hot or cold air completely change the refraction from that given in the standard tables. During an eclipse the heat of the sun is withdrawn from a funnel shaped column of air over the observing station, and this must cause some local and irregular changes of temperature, changes in the upper regions of the atmosphere as well as near the surface. Such abnormal conditions of the atmosphere may well give rise to abnormal refractions, refractions in azimuth as well as in altitude. Abnormal changes in the temperature of the column of air through which the rays passed amounting to only a very few degrees would change the course of the rays by an amount larger than the sought-for Einstein effect. And such abnormalities can neither be computed nor allowed for.

Now a photograph of the stars taken at the time of an eclipse cannot, by itself, give any result. It must be compared with a similar photograph, taken with the same instrument at some season of the year when the sun is in a different portion of the sky. Such plates were taken at Sobral during the month of July, or some fifty days after the eclipse; when, in the early morning hours, the altitude of the star group was approximately the same as on the day of the eclipse. During this interval of some fifty days the telescopes were left on their mountings and in place, but the

mirrors and the driving clocks were dismounted and put into a house to avoid exposure. Thus the comparison plates were taken under almost, but not identically, the same instrumental conditions as were the eclipse plates. Of course the atmospheric conditions were different, the temperature during the time of the eclipse averaging some 82°.7F., while it was only 72°.9F. at the time the check plates were taken. But on the whole, the conditions, instrumental and atmospheric, were as near alike at the times of taking the Sobral eclipse and comparison plates as it was possible to have them.

The case is very different for the Principe expedition: the comparison plates were taken at Oxford in January and February, under radically different conditions of altitude and temperature. And further, during the interval between the times of taking these comparison plates and the day of the eclipse, the instrument had been completely dismantled, transported some thousands of miles, and remounted in an entirely different manner. Any results, credited to the Principe expedition, are, therefore, subject to all the uncertainties due to such radical differences of instrumental and atmospheric conditions.

Now to discuss the eclipse plates themselves. At Principe clouds seriously interfered with the work. In the morning there was a heavy thunder-storm, and from that time on the sun was obscured by thick

clouds, which gradually grew thinner. Half an hour before totality the sun could be glimpsed occasionally, and by the time the critical moments arrived it could be seen continuously through the drifting clouds. The time of totality had been carefully computed, and when this moment arrived the programme of exposing plates was carried out. In all sixteen (16) plates were obtained, giving fine pictures of the inner corona and of a remarkable prominence on the edge of the sun. But star images appeared on only seven (7) of the sixteen plates, and these seven varied greatly: on some of them the images are noted as "good"; on others as "diffused" or "very faint." For the practical determination of the sought-for "light deflection," the images of three stars, Nos. 3, 4, and 5, are necessary, and the images of these three stars appear together on only four (4) plates; on two of which No. 5 is noted as "faint, diffused." This left only two plates out of the sixteen as likely to give trustworthy results. Measures were made on the other plates, but the images gave discordant results.

Thus the so-called Principe results depend solely upon two (2) plates taken under very unfavorable conditions.

At Sobral, as has been seen, there were two instruments, the regular 13-inch astrographic lens and the 4-inch auxiliary camera. On account of defects, which had been discovered in the large cœlostat mirror, the

astrographic lens was stopped down to 8-inches aperture. On the morning of the eclipse day the sun was covered with clouds: at the time of first contact the sun was invisible. As the eclipse progressed towards the total stage, however, the clouds diminished, and during the critical period of totality the region of the sky in the vicinity of the sun was practically free from clouds. During the middle of this period, unfortunately, a thin cloud passed over the sun; not sufficiently thick to conceal the corona, but heavy enough to prevent photography of the stars. With the astrographic telescope nineteen (19) plates were taken, with alternate exposures of 5 and 10 seconds; with the 4-inch camera eight (8) plates were taken, with a uniform exposure of 28 seconds. When these plates were developed, it was found that at least seven stars appeared on sixteen (16) of the plates taken with the large telescope, and upon seven (7) of the plates taken with the 4-inch camera. The remaining plates, taken through the thin film of cloud, did not show the necessary star images.

The plates taken with the astrographic lens were not good. This was seen as soon as the first plates were developed, as the following note will show:

"May 30, 3 a. m., four of the astrographic plates were developed, and when dry examined. It was found that there had been a serious change of focus, so that, while the stars were shown, the definition was spoilt. This change

of focus can only be attributed to the unequal expansion of the mirror through the sun's heat. The readings of the focussing scale were checked next day, but were found unaltered at 11*mm. It seems doubtful whether much can be got from these plates."*

The results justified this note, which was made at the time. The images on all the plates were diffused and apparently out of focus. The exact cause of this apparent change of focus could not be determined, but it was attributed by the astronomers of the expedition to the action of the sun's heat upon the 16-inch mirror, which reflected the sun and its surroundings into the lens of the telescope. The plates, however, were all duly measured, but the results were unsatisfactory and were given but little weight by the astronomers in their final conclusions. The Report sums up the value of these plates in the words:

*"The photographs taken with the astrographic telescope support those obtained by the '*4*-inch' to the extent that they show considerable outward deflection, but for the reasons already given are of much less weight."*

When, therefore, the "Sobral Results" are alluded to one must understand that the results obtained from the seven plates taken with the 4-inch camera are alone meant. One of these is shown in the accompanying plate, which has been reproduced from the plate contained in the Report. It is from an untouched negative,

but, of course, suffers in the double reproduction. The disc of the moon appears white, and the brilliant corona, which surrounds the concealed sun, is shown black, in reverse. The images of the stars are the minute black specks, between the dashes. These dashes are the only modifications of the original plate, and they were put on solely to call attention to the various stars, which otherwise would escape notice. It will be noted that the image of one star is clearly within the limits of the corona, itself; while two more are on the border, where the plate appears faintly discolored by the light of the corona.

This plate and the six similar ones were measured in the manner heretofore described, and similar measures were made on seven comparison plates, taken during July. After correcting the measures for differential refraction, aberration, orientation, and change of scale, the mean or average deviation from the standard point of reference was found for the seven images of each star on the eclipse plates; and the similar mean deviation for the seven corresponding images on the seven comparison plates. Assuming that the mean position, or deviation, as shown on the comparison plates represents the true position, or deviation of the star, then the difference between this mean and that for the eclipse plates gives the shift in position, or the "light deflection," due to the presence of the sun. All of this is shown on the following diagram, where the compara-

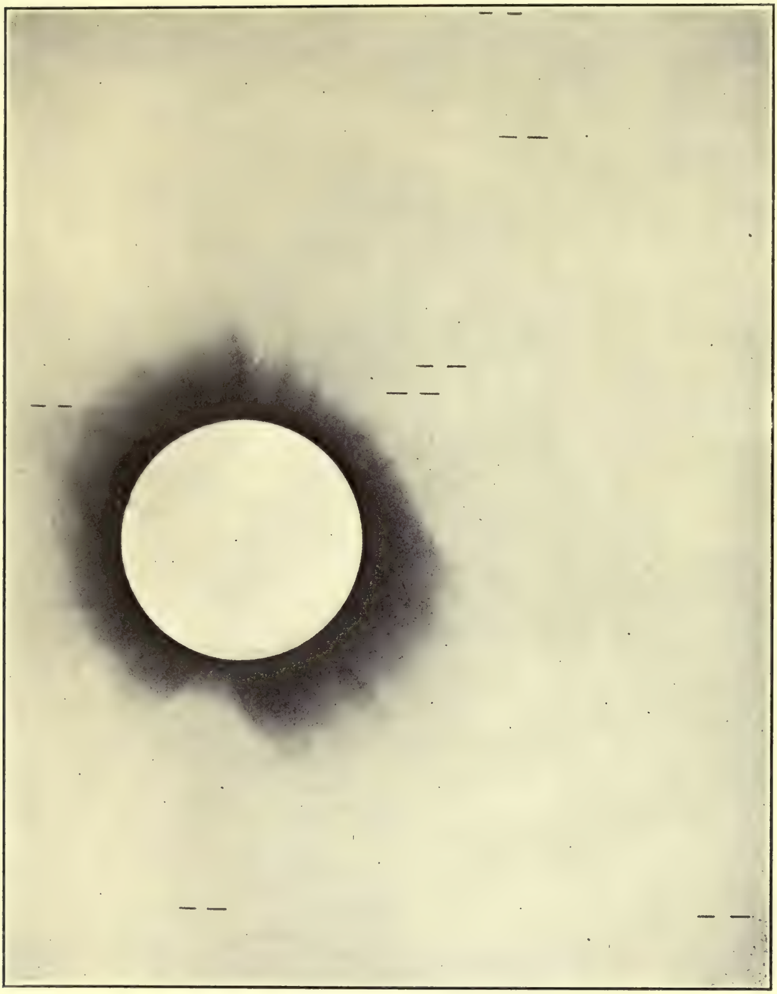


Plate 3.

The Eclipse of the Sun: a photograph taken at Sobral, Brazil, May 29, 1919.

This is an untouched reproduction of the original negative except for the dashes, which were put on to call attention to the minute star images. The Corona, which consists of very tenuous matter surrounding the Sun, is clearly seen extending out to and enveloping the nearest star. Einstein neglects this matter entirely in all his theories, and claims that it can have no refractive effect upon the light which passes through it, and no effect upon the motion of Mercury.

tive positions of each one of the stars, as it appears on all fourteen plates, are shown. The small dots represent the various positions of the star as deduced from each one of the seven eclipse plates, the number beside each dot is the number assigned to the plate in the Report. The point of the heavy arrow is the mean or average position, as determined from all the plates. Similarly, in each figure, the small crosses represent the positions of the same star as found on the seven comparison plates: the notch of the arrow, the mean position. The observed deflection is the difference between the two mean positions, and is shown by a heavy arrow: the theoretical Einstein shift is shown by a broken arrow.

In order to bring out more clearly the relative accuracy of the eclipse and comparison plates, circles are drawn enclosing for each star the eclipse positions and the comparison-plate positions, respectively. The circles for the eclipse plates are drawn with a full line; those for the comparison plates with a broken line. It will be noted that, with a couple of exceptions, the comparison plates are in much better accord than the eclipse plates: the groups of crosses being much more condensed than the groups of dots. In three out of the seven cases, the circles overlap.

The plotted results, Figure 26, show clear evidence of the effect of the shadow cone, heretofore mentioned, upon the observed positions of the stars. Plate No. 1 was taken at the beginning of totality; Plate No. 8,

just before the eclipse ended. In the case of every star, the observed or measured deflection is smaller on Plate 1 than on Plate 8, and in most cases this difference is extremely marked. This increase in the measured deflection from Plate 1 to Plate 8 is very clearly brought out by dividing the plates into three groups, and by taking the average deflection of all the stars on all the plates in each group. The first group contains Plates 1 and 2; the second, or intermediate, group Plates 3, 4, and 5; the final group the last two plates, Nos. 7 and 8. The average deflections, thus found, are:

Plates 1 and 2	0″.30
Plates 3, 4 & 5	0″.34
Plates 7 and 8	0″.38

The general average of all the stars on all the plates is 0″.34. There thus appears to be an average increase in the size of the observed deflection of some 27% as the eclipse progressed.

Again, the directions of the observed shifts show changes with the progress of the shadow cone; there are marked differences between the first and last groups of plates in the cases of six out of the seven stars.

For each star there are seven positions, one from each of the seven eclipse plates, and seven comparison positions from the seven comparison plates. From each

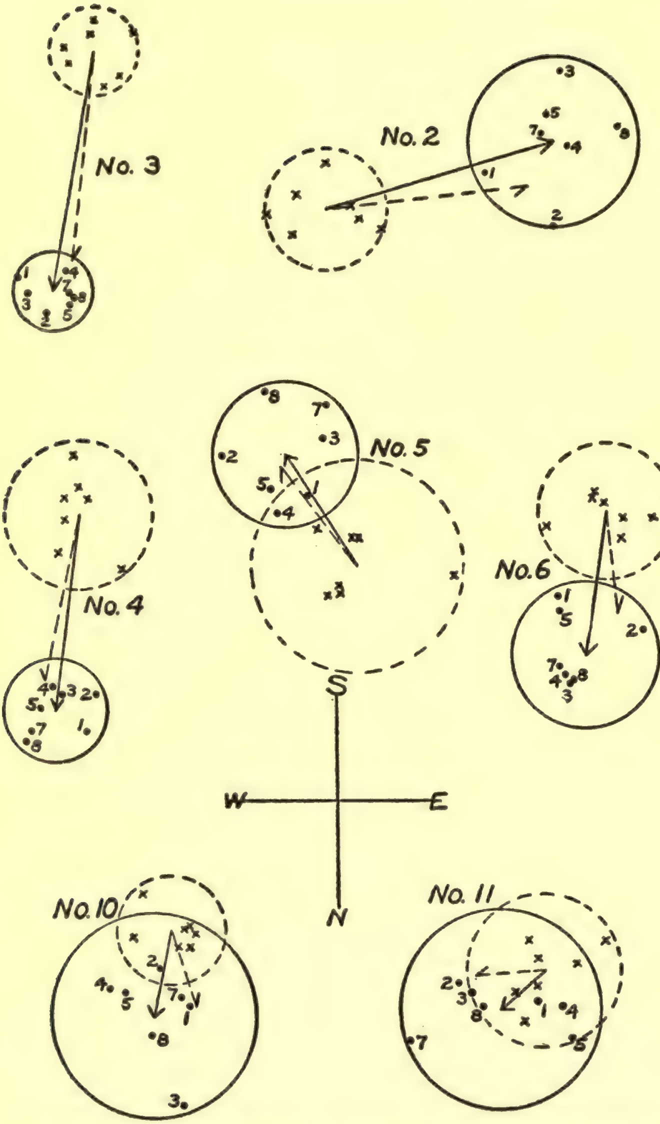
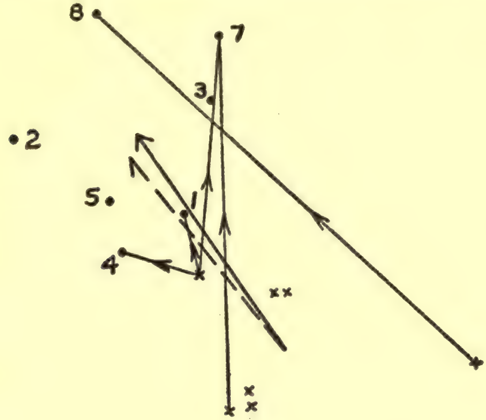


Fig. 26. Individual Results of Eclipse and Comparison Plates.

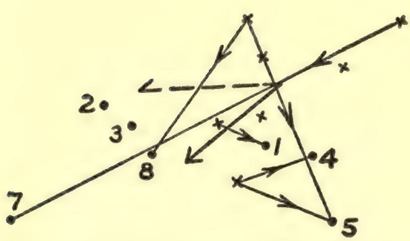
possible combination of an eclipse position with a comparison position could be derived an observed deflection: in all there are forty-nine such combinations for each star. These forty-nine combinations, or observations differ very materially among themselves. Some of these actual observations are shown in the annexed diagram: some for star No. 5, which agrees most nearly with the Einstein prediction, and some for star No. 11, which gives the most discordant results. In each case the thin lines show the actual observations, the heavy broken arrow the predicted Einstein effect, and the full heavy arrow, the mean observed deflection as given in the report of the British astronomers. In order not to complicate the figures too greatly, only a few of the forty-nine observations are shown in each case. The figure for star No. 5 shows that the directions of the individual observations differ by approximately 36° on each side of the Einstein arrow, and that the observed amounts differ from 0″.20 to 1″.03, or by a trifle over 500%. The figure for star No. 11 shows that the directions vary all the way from − 135° to + 160°, or a total variation in direction of practically 300°; the distances vary from 0″.10 to 0″.90.

Further, a glance at these diagrams will show how the omission of one or two plates would radically change the average or mean result, as given in the Report. In the case of star No. 5, one of the comparison positions is extremely discordant: if this one

plate be omitted and the average of the remaining plates taken, then the supposed observed deflection would be



No. 5 – The best Star



No. 11 – The worst Star

Fig. 27. The Discordant Results: Stars 5 and 11.

greatly shortened, and the direction of the apparent shift would be changed, so as to differ radically from the Einstein direction. In the case of star No. 11, if

some of the plates be omitted, the direction of the observed shift would be almost directly opposite to that predicted by Einstein.

The British astronomers selected seven out of thirty-three plates and obtained from these seven their results. If this process of selection be pushed still further and only two or three plates taken, then almost any desired result could be obtained: the deflection could be made to appear almost anything, in direction and in amount.

The mean results from all the plates are summarized in the following table, which is taken directly from the Report. In this table the stars are arranged in the order of their respective distances from the sun, and the radial component of the observed deflection is given in each case.

TABLE III

Radial Displacement of Individual Stars

Star	Calculation	Observation
11	0″.32	0″.20
10	0″.33	0″.32
6	0″.40	0″.56
5	0″.53	0″.54
4	0″.75	0″.84
2	0″.85	0″.97
3	0″.88	1″.02

This table shows that, on the average, the observed deflection, as given by the British astronomers, differs by 19% from the calculated Einstein value. In the

cases of two stars the agreement between theory and observation is nearly perfect, the observed value being only 3% in error: in other cases, however, the differences range from 11% to 60%. Not only are the actual amounts of the deflections, as observed, for the individual stars, thus different from the theoretical Einstein values, but the rate of decrease from star to star is radically different from that predicted. The difference between the deflection of the star nearest the sun and that of the farthest star should be, according to Einstein, 0″.56; while the observed or measured difference was 0″.82, practically 50% out of the way.

The diagrams, given above, show clearly that the observed displacements of the stars do not agree in direction with the predicted Einstein effect. This point was nowheres mentioned in the Report, which took up only the amount of the radial component of the actual displacement. But, after the measurements of the plates became available for study, several investigators called attention to this fact of a radical disagreement in direction between the observed and predicted displacements. These differences in direction amount to many degrees, in the case of the star farthest from the sun to 37°. Thus, even the seven best plates out of the thirty-three, which showed star images, give inconsistent results:—the observed shifts in the star images, if real, do not coincide with the Einstein effect either in amount or in direction.

But these seven plates clearly indicate shifts, or displacements, of the star images, and, in a very general way, these shifts agree somewhat both in direction and amount with the predicted Einstein effect. It has been claimed by many that the differences between the observed and predicted shifts are no greater than should be expected; that the differences, as shown in the diagrams, are the mere unavoidable errors of measurement and calculation which creep into every physical or astronomical research. If these differences are within the limits of allowable accidental errors, and if the displacements are real, then these plates certainly furnish some prima facie evidence in favor of the relativity theory. But, if these differences are real and greater than allowable accidental errors, then the case is altered, and the evidence is nowheres near as strong, for then there is a distinct, proved disagreement between observation and prediction. Now this very question was investigated by Dr. Henry Norris Russell, of Princeton University, a most ardent upholder of the relativity theory. He studied these star displacements with a view of determining whether the departures from the Einstein predicted effects are real or not, and, if real, of finding some possible explanation of them. As a result of an exhaustive examination of them, he concludes that these differences between the observed and the predicted displacements, these non-Einstein displacements, as he calls them, are *real,* and cannot be

attributed to mere accidental errors of observation and measurement. He finds that the Einstein effect *alone* will not represent the observations; that, in order to represent satisfactorily the measurements from the plates, it is necessary to supplement the Einstein prediction by a distortion of some kind.

Dr. Russell assumes that the most probable source of these proved non-Einstein deflections is to be found in instrumental errors: in an alteration in the shape of the mirror, caused by the heat of the sun. He finds from an elaborate investigation the form and amount of mirror distortion that will best satisfy these non-Einstein effects, and concludes that, if the mirror had been distorted into a cylindrical form with a definite radius of curvature, then the non-radial displacements can be well accounted for. The sun's heat undoubtedly distorted the mirror, and it is highly probable that some portion of the observed displacement, radial as well as non-radial, was due to such distortion. The mirror of the large instrument was so affected, and the nineteen plates taken with that instrument were very unsatisfactory, and were practically discarded by the British astronomers. It has been known for many years that the horizonal form of instrument is utterly unsuitable for exact measurements; a fact tacitly admitted in the concluding paragraphs of the Report.

But one point is perfectly clear. If it be admitted that the heat of the sun so distorted the mirror of the

apparatus as to cause errors of 20%, in some cases of 50%, of the measured displacement, then the entire set of plates is worthless for proving the existence or non-existence of the "Einstein effect." Following the methods used by Russell it might not be impossible to find, by suitable adjustments of radii of curvature and axes, a distorted shape of the mirror that would explain the entire observed deflections.

Either these observations represent actual displacements of the star images, or they represent mere instrumental distortions: either they are good and must be explained in their entirety; or they are worthless and must be discarded. If these deflections of the star images on the plates are *real,* and are not due to distortions of the photographic film, or to defects in the photographic apparatus, then Russell has shown clearly that they cannot be explained in their entirety by the Einstein theory. That theory will account for deflections in the star images, but not for the *observed deflections.*

The Einstein theory calls for a deflection of light amounting to 1″.75 at the edge of the sun, and decreasing proportionally with the distance from the centre, so that for a ray passing the sun at a distance of one radius from the edge, or two radii from the centre, the deflection should be exactly one-half the above amount. From this law of decrease and the known distances of the star images from the centre of the

sun, the observed deflections can readily be transformed into what they would have been at the sun's edge. This is done in the Report, and the average result of all the stars taken. In finding this mean, the measurements in declination were given twice the weight of those in right ascension. The Report gives these final, or mean, results for the three separate sets of observations as·

From the seven (7) plates taken at Sobral with the 4-inch camera,

$$1''.98$$

with a probable error of about $\pm$ 0''.12.

From the two (2) plates taken at Principe with the 13-inch astrographic lens

$$1''.61$$

with a probable error of about $\pm$ 0''.30.

From the sixteen (16) plates taken at Sobral with the 13-inch astrographic lens

$$0''.93$$

"For reasons already described at length not much weight is attached to this determination." *

These results, in each case, are the means of the radial components only; nothing whatever being given as to the directions in which the actual displacements took place. The Einstein theory requires a deflection, not only of a certain definite amount, but also in a

* Report.

certain definite direction. To discuss the amount of the observed deflection is to discuss only one-half of the whole question, and the less important half at that. The observed deflection might agree exactly with the predicted amount, but, if it were in the wrong direction, it would disprove, not prove, the relativity theory. You cannot reach Washington from New York by travelling north, even if you do go the requisite number of miles. Now, the diagrams, above given, of the seven best plates, the seven taken at Sobral with with 4-inch camera, show clearly and definitely that the observed deflections are not in the directions required by the Einstein theory. In the case of star No. 10, the observed mean deflection differs in direction by some 28° from the predicted direction: not only that, but every one of the seven plates shows the star deflected in the same direction from that called for by the relativity theory; every star dot, in the diagram, lies on the same side of the Einstein arrow. Similarly for star No. 11, every dot again lies on the same side of the Einstein arrow, and the mean deflection differs by 37° from the predicted. In this case two of the individual plates give deflections practically in the reverse direction to that called for by the theory. The best agreement between theory and observation is given by star No. 4, where the mean difference amounts to about a single degree: but, even in this case, the individual results differ by as much as 30°.

The relativitist either totally disregards these discordances in the directions of the observed deflections, or invokes the heating effect of the sun to distort the mirror by just the proper amount to explain them away!

Again, disregarding directions entirely, and taking into account only the size of the deflection, it is noted that the disagreement between the three mean results, as given in the Report, is over 100%; the largest value being well over twice that of the smallest. The actual amount of the deflection as obtained with the astrographic lens is 58% of that obtained at Principe and only 47% of that of the 4-inch camera at Sobral. This difference in results is far beyond the limits of accidental errors. Distortions in the mirror of the large cœlostat, due to the heating effects of the sun, are called upon to explain this discordance. But, in this case such procedure is absolutely justifiable, for the effects of the distortions were noted in the negatives, before any of the plates were measured. But the fact that one set of plates was ruined by such heating effects, renders the other set, taken under similar conditions, at least of doubtful value.

When the deflections of light, as actually observed, are considered both in direction and in amount, the discordances with the predicted Einstein effect become marked, and the plates present little or no evidence to support the relativity theory. Further, if these deflec-

tions are real, and not due to instrumental errors (so readily called upon by the relativitist to explain everything that the relativity theory cannot account for), then it has not yet been shown that the relativity theory is the only possible explanation. As a matter of fact there are other perfectly possible explanations of a deflection of a ray of light; explanations based upon every-day, common-place grounds. Abnormal refraction in the earth's atmosphere is one; refraction in the solar envelope is another. The atmospheric conditions under which the eclipse plates were taken were necessarily abnormal, and the plates, themselves, clearly show that the rays of light passed through a mass of matter in the vicinity of the sun; a mass of density sufficient to clearly imprint its picture upon the photographic plates.

Such is the evidence, such are the observations, which, according to Einstein, *"confirm the theory in a thoroughly satisfactory manner."*

CHAPTER VIII

THE OBSERVED PHENOMENA AND CLASSICAL METHODS

THE former chapters show clearly that the Relativity Theory is inadequate to explain either the observed motions of the planets, or the observed deflections of light rays: it can account for the motion of the perihelion of Mercury and for a certain definite deflection of light, but it cannot account for other observed motions of Mercury and Venus, nor for the light deflections as actually observed. The relativitist is forced, either to deny the existence of these other motions, or to supplement his theory by some other agency to account for those things, which the relativity theory by itself cannot explain. In the words of the mathematician, the relativity theory, alone, is not sufficient.

On the other hand, the ordinary classical methods of physical and astronomical research can fully explain all the observed phenomena; the motions of the planets can be fully accounted for by the presence of matter known to exist, and the light deflections, if real, can

be explained as the result of refraction through the cosmic dust surrounding the sun. It is true that Newcomb, in forming his tables of planetary motion for the Nautical Almanac, dismissed the idea of unknown masses of matter and introduced arbitrary corrections to take care of the observed discordances; corrections, however, so introduced as to commit him to no hypothesis as to the cause of the observed excess motions. Newcomb was largely influenced by the desire for simplicity, or rather a desire to avoid a "complication in the tabular theory." His investigations, heretofore fully outlined, showed that no one single planet, nor a single group of planetoids, could fully account for all the observed discrepancies, although a group of planetoids between Mercury and Venus more nearly satisfied the conditions than any other hypothesis.

The Motions of the Planets:

Simplicity may be desirable, but it is not essential. It was undoubtedly the desire for simplicity, the wish to find a single cause for the sometimes conflicting motions, that led Newcomb and other investigators into their seeming difficulties. Yet it is hardly likely that all the six or seven small discordant motions of the various planets have the same cause. There are several possible, even probable, causes for each and every one of the discordances, and the true explanation

probably lies, not in a single cause, but in a combination of causes; not in a single unknown, unseen planet, or a single group of planets, but in a combination of groups, or an irregularly scattered mass of matter about the sun. The sun is known to be non-homogeneous, and it is known to be surrounded by a vast irregular mass of matter, by an envelope extending far beyond the orbit of the earth. In the fact of this irregularity of the sun itself and in the presence of this matter can be found full and satisfactory explanations of all the various motions.

The sun is known to be a rotating, cooling mass of gas, and fundamental laws of physics show that such a body must be a spheroid. The equatorial diameter of the sun must, thus, exceed the polar by an amount not less than $0''.05$, a quantity far below the limits of possible measurement. As has been seen in a former chapter, the actual measurements indicate an ellipticity somewhat larger than this, an ellipticity just verging on the limits of measurement at around $0''.10$. Such an ellipticity in the sun would cause a motion of $7.''0$ in the perihelion of Mercury, and corresponding, although very much smaller, motions in the perihelia of the other planets. This is certainly a possible, rather an extremely probable, cause of a considerable portion of the unexplained discordance. And there is nothing at all new or unique about such an explanation. It has been in use for well over one

hundred and fifty (150) years to explain and account for similar motions among the satellites of Jupiter. The disc of Jupiter is clearly elliptical, and as early as 1748 Euler showed that such elliptical figure would cause irregularities in the motions of the satellites, and in 1758 Walmsley showed that it would produce a rotation of the orbit of the satellite precisely similar to the now much discussed motion of Mercury. Since that date it has been an accepted method of Celestial Mechanics to account for perihelial motions of the various satellites of the Solar System by means of the observed and measured ellipticity of the planet about which they revolve. The elliptic shape of the earth accounts for several of the observed irregularities in the motion of the moon.

But the elliptic shape of the sun will not account for more than a small portion of the discordance in the motion of Mercury's perihelion, and will not reconcile the observed differences in the other motions of both Mercury and Venus. Such remaining discordances must have other causes, and such causes can be found in the immense envelope of matter, which surrounds the sun and which extends far beyond the orbit of the earth. Unfortunately the exact distribution of this matter throughout space is unknown; and, therefore, its effects upon the motions of the planets cannot be accurately calculated. While, thus, the problem fails of a direct solution, yet it is possible to attack it

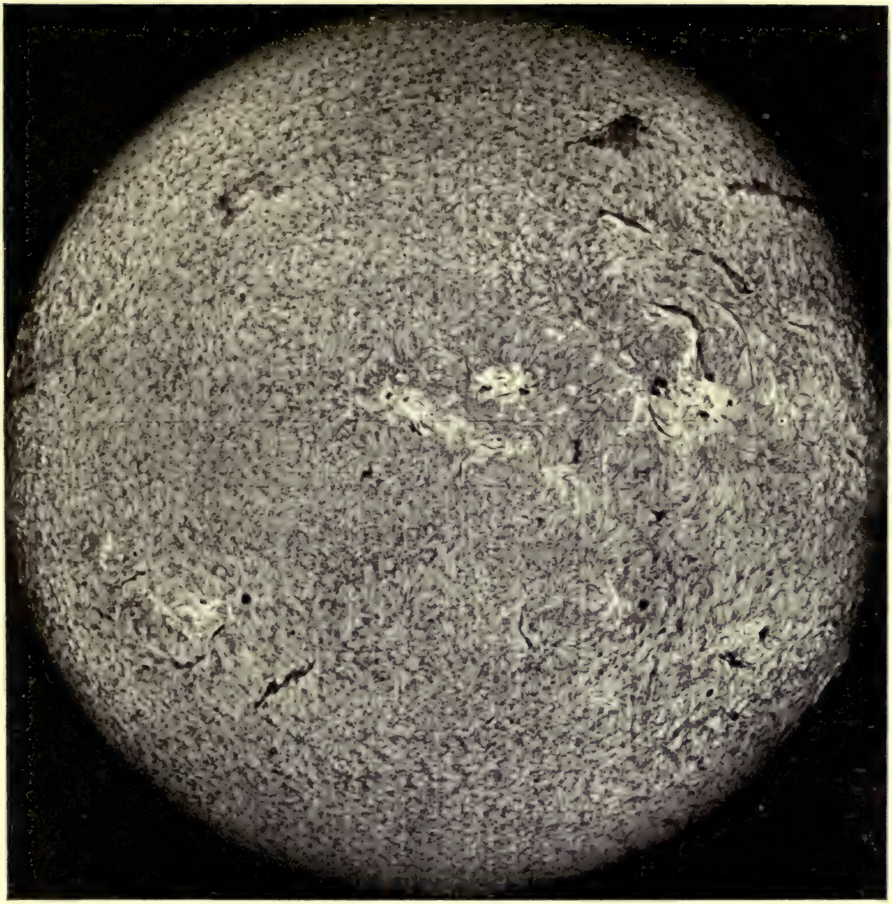


Plate 4.

The Sun, photographed in the light of glowing Hydrogen at Mount Wilson Observatory: vortex phenomena near the spots are especially prominent.

The so-called astronomical proof of the Einstein theory is based upon the assumption that the Sun is a perfect sphere, each concentric layer of which is of uniform density. This photograph clearly shows that the Sun is a mass of whirling, rising and falling gases; never at rest and never uniformly distributed. The actual condition of the Sun, as shown by this photograph, is totally disregarded by Einstein.

in reverse. That is, it is possible to find what distribution of this matter would, under the Newtonian law, give rise to the discordant motions as actually observed. Then this calculated distribution can be compared with the known facts to determine whether or not it conforms to observation and is within the bounds of reason. This problem is similar to that solved by Adams and Leverrier, which resulted in the discovery of Neptune. It will be remembered that these eminent astronomers calculated, from certain observed irregularities in the motions of the planet Uranus, the positions and motions of an hypothetical planet, which would cause such motions; and that, subsequently, a planet, now known as Neptune, was found very close to the predicted place.

Now in a very general way it is known that this solar envelope is lens-shaped and of very minute density; more dense in the immediate vicinity of the sun than in its more remote extensions beyond the orbit of the earth. Observations of the zodiacal light show that, in the outer portions, the principal plane of this lens of matter does not differ radically from that of the earth's orbit; while photographs of the corona show that, in the immediate vicinity of the sun, this plane does not differ greatly from that of the sun's equator. This general distribution can be approximated to by assuming the whole mass to be made up of ellipsoids of revolution, each ellipsoid to

be of uniform density, but the larger ones to be of much less density than the inner ones. Some such assumption is necessary to reduce the problem to the realm of figures: the selection of ellipsoids of revolution is naturally indicated by the fact that all the known bodies of the solar system are such ellipsoids.

An ellipsoid, or ring, of matter wholly within the orbit of a planet will give a direct motion to the perihelion. But if the orbit actually lies in the matter composing such ellipsoid, then the effect is the opposite and the motion of the perihelion will be retrograde. This, of course, upon the assumption that the density is uniform throughout; if the density is much greater in the central portions of the ellipsoid, then the retrograde effect of the outer portion may be overcome and the total effect upon the perihelion may be direct, but the motion will be less than that due to the central portion alone. By adjusting the rate at which the density is assumed to decrease, any motion of the perihelion, direct or retrograde, within limits can be obtained. To changes in the density of the envelope surrounding the sun may thus be attributed the discordant motions of the perihelia of the four inner planets, and especially the retrograde discrepancy in the motion of Venus.

For purpose of computation the entire mass may be supposed to be made up of three superimposed ellipsoids, each of constant density. This merely makes

the changes in density abrupt, instead of gradual. These three are:—

a. A small central ellipsoid entirely within the orbit of Mercury. The position of this ellipsoid in space was determined from the discordances themselves.

b. An intermediate ellipsoid entirely within the orbit of the earth, but extending beyond the orbit of Venus. The principal plane of this was assumed as being the same as that of the orbit of Jupiter.

c. An outer ellipsoid entirely within the orbit of Mars, but extending beyond the orbit of the earth. The principal plane of this was also assumed as being the same as that of Jupiter's orbit.

The effect of each ellipsoid upon the perihelia, the nodes, and the inclinations of the planets can be found by formulas of Celestial Mechanics, and the positions and densities of those ellipsoids, which will best account for all the motions, can be determined. No distribution can be found that will rigorously satisfy all the motions, but the positions and densities of those ellipsoids can be found, which will approximately satisfy all the equations and practically account for all the discordances in the motions of the planets. The annexed table shows with what a high degree of accuracy the motions of the planets can be accounted for under the action of the sun and this widely distributed matter. For purposes of comparison the residuals on the basis of the Einstein theory are also given:

TABLE IV

Final Discordances in the Motions of the Planets

	Amounts	*Final Discordances:*	
PERIHELIA:	to account for:	Einstein	Poor
(1) Mercury	+ 39″.8	− 3″.0	+ 0″.1
Venus	− 7″.3	− 15″.9	+ 0″.2
Earth	+ 5″.9	+ 2″.1	+ 0″.3
(3) Mars	+ 8″.1	+ 6″.8	+ 3″.1
NODES:			
Mercury	+ 5″.1	+ 5″.1	+ 0″.6
(2) Venus	+ 10″.2	+ 10″.2	− 1″.5
ECCENTRICITY:			
(4) Mercury	− 0″.9	− 0″.9	− 0″.2

The relative probabilities of two theories, or two solutions of a problem, are usually determined from the final differences, or residuals, as these differences are called. That solution is deemed the more probable which makes the sum of the squares of the residuals the smaller. If this test be applied to the residuals in the above table, the results are:

Einstein theory	473
Sun and Solar envelope	14

And these clearly indicate how very much more probable is the explanation of the motions of the planets as due to the presence of matter in space, than as due to the hypotheses of Einstein.

Now in order to determine whether or not such a distribution of matter, as called for in the above solution, is possible, some computations must be made as to its necessary mass and density. The formulas, however, are such that the mass or density of each ellipsoid depends upon its radius, and cannot be independently determined. The smaller the ellipsoid, the greater mass it must contain in order to account for the various motions. But the radii can be arbitrarily assumed, and the corresponding masses and densities found. Taking the shape of the ellipsoid to be such that the axis about which it revolves is only 1/10th of the larger radius, and supposing the inner ring, or ellipsoid, to extend some forty (40) radii of the sun, or approximately to one-half the distance of Mercury, and the second ellipsoid to be just a trifle smaller than the orbit of the earth, we have the following:

	Mass	Density
Inner ellipsoid	3	8.9×10^{-8}
Second ellipsoid	4/7	1.3×10^{-10}

where the density of air is unity and the masses are given in terms of that of Mercury.

The density of the air is measured by its pressure, and the standard at sea-level is 760 mm. of mercury. The pressure of this matter in space would be measured by one fifteen thousandth (1/15,000th) of a millimeter.

Now to obtain some idea as to what this density really means, suppose that one-half of the total mass of the inner ellipsoid be concentrated into a ring of planetesimals of an average diameter of fifty (50) miles; the outer edge of the ring being at the outer limits of the ellipsoid, or one-half the distance of Mercury from the sun. If the cross-section of such a ring contained one hundred (100) such planetesimals, that is, if it consisted of ten rows of bodies, one above the other, each row containing ten, then the average distance between these bodies would be some 17,000 miles. Individually such bodies would be invisible, for they would never be more than 15° from the sun, and a body of such small diameter would be completely lost in the glare. At the same angular distance from the sun, Mercury, 3000 miles in diameter, is a difficult object. The only possible chance of discovering any one of such a group of bodies would be at the times of total solar eclipses, and even at such times the chances of actually seeing such a small body would be almost infinitesimal.

In order to account for the motions of the nodes, the inclination of such a ring, or ellipsoid, of planetesimals would have to be comparatively large. Computation shows that the inclination to the ecliptic of such a ring would be between 7° and 8°. Newcomb, in his discussion of the general subject many years ago, considered such a great inclination as highly improbable, believing that such a group of bodies would tend to gather

around a plane somewhere between that of the orbit of Mercury and that of the invariable plane of the planetary system; that is, between 7° and 1°. The inclination of Mercury's orbit is 7° and that of sun's equator is 7° 15′, and it would appear more reasonable to expect a group of small planets in the immediate vicinity of these two bodies to have inclinations somewheres near the two. Further, if Newcomb's reasoning be correct, then the great ring of planetoids, between Mars and Jupiter, should certainly show a distinct grouping near the plane of Jupitor's orbit, for these bodies are at a remote distance from the sun and Mercury, and are in the immediate vicinity of Jupiter itself. Yet the individual planetoids, which form this ring, have inclinations varying all the way up to 35°. The four largest of the group have inclinations of 10°, 34°, 7°, and 13° respectively. Thus, in this matter, Newcomb's reasoning fails in a case very much more favorable to his theory.

There is apparently no mechanical nor physical reason for the non-existence of a group, or groups, of bodies, sufficient to explain all the irregularities in the motions of the planets. Thus, all the discordances, including that of the perihelion of Mercury, can readily be accounted for by the action, under the Newtonian law, of matter known to be in the immediate vicinity of the sun and the planets.

It is, however, possible that the Einstein hypotheses

be true, and that the discordant motions of the planets result from a combination of the Einstein motions and the effect of the widely distributed matter in space. Just as a definite distribution can be found which will explain the discordances given by Newcomb, so also another and different distribution can be found that will more or less fully account for the discordances remaining after applying the Einstein effects. And this distribution is not radically different from that found above, except that the density of the matter is more nearly uniform throughout space; it is less dense near the sun and more dense in the vicinity of the earth's orbit. In fact the corresponding ellipsoids would be:—

	Mass	Density
Inner ellipsoid	1/4	7.4×10^{-9}
Second ellipsoid	5/7	1.5×10^{-10}

in which as before the unit of mass is Mercury and that of density, air at standard pressure.

Thus the motions of the planets do not prove the truth of the Einstein theory, nor, on the other hand, do they prove its falsity. While these motions can be accounted for by a certain distribution of matter in the solar envelope, it has not yet been established by observation that the matter is distributed through space in the required way. In the present state of our knowl-

edge regarding this matter, the motions of the planets do not and cannot furnish a definite answer to the question as to the validity of the relativity hypothesis. It is then a problem of observational astronomy to investigate the actual distribution and density of the matter in the solar lens, and to determine whether or not it approximates the conditions necessary to account for the planetary motions.

But one conclusion is certain, the Einstein hypotheses and formulas are neither *necessary* nor *sufficient* to explain the discordances in the planetary motions.

THE SOBRAL ECLIPSE PLATES:

The curvature of light rays, supposed to have been clearly proved by the eclipse photographs, may or may not exist. The seven Sobral plates show clear evidence of shifts in the star images, but it has not been shown that such shifts are in fact due to the bending of the rays at the sun. Such apparent shifts may be due to instrumental errors, to distortions of the mirror, to abnormal refraction in the earth's atmosphere caused by the cooling effect of the passing shadow cone.

But, if the deflections are real and are caused by a bending of the rays of light at or near the sun, such bending may be due to perfectly natural causes. The Sobral photographs show clearly that the rays of light, in their course from the distant stars, passed

through masses of matter near the sun. This matter was sufficiently dense and reflected enough sunlight to imprint its image upon the photographic plates, and there can be no question as to its existence and its presence in the paths of the light rays. Further, whenever a ray of light passes from free space into, or through a medium of any kind or density, such ray is refracted, or bent out of its straight course. The path of such a ray becomes curved, and the amount of refraction, or curvature, depends upon the density of the medium into which the ray passes and the angle at which it meets the surface. This is a fundamental law of physics: upon the refractive effects of different media are based our optical instruments and experiments: eye-glasses, cameras, microscopes, telescopes; all depend upon the refractive effect of glass upon a ray of light. It is certain, therefore, that the rays of light, in passing through the solar envelope, suffered a refraction, or bending, of some kind and amount. This fact is as well established as the existence of the sun itself. The sole question is whether this refraction was sufficient in amount and in direction to account for the observed displacements of the star images.

This possibility of accounting, in a perfectly normal way, for the observed light deflections has been dismissed by the relativitist in a few words as a matter scarcely worth mentioning.

While it is certain that the rays suffer some refraction

in passing through the solar envelope, it is claimed by most astro-physicists that the effect is so small as to be negligible in comparison with the observed deflections. This idea is so firmly fixed that the possibility of explaining any portion of the deflections by refraction was dismissed by the British astronomers in their Report with a scant phrase or two. The entire question depends upon the possibility of the solar envelope having a density large enough to bend a ray of light by the required amount, and this in turn upon what that density really is.

It can readily be shown by the ordinary formulas of optics that a lens of matter of a density of about 1/140th that of air at standard pressure and temperature would deflect a ray of light by about 1″, the amount observed in the case of the star nearest the sun. And this is the density that the astro-physicist assumes to be necessary to account for the observed deflections. And, of course, matter of such density cannot exist in the immediate vicinity of the sun, where the light rays passed. This is not a physical impossibility; there is no fundamental law of physics or mechanics that renders such a density an impossibility; rather is it an observational improbability, made so by other, definite observations. Such matter, if it existed, would refract, or scatter, the rays of the sun itself in all directions, and the sun would appear surrounded by a large, brilliant halo, or corona. There is a mathematical relation,

depending upon physical laws, between the density of the matter and the size and brilliancy of the surrounding halo of the sun. The actual corona of the sun, as observed and photographed, would be far larger and far more brilliant than it really is. From estimates of the brightness of the solar envelope at the points where the rays passed, the physicist concludes that the density of the envelope cannot be greater than one one-thousandth part of that deemed necessary to produce the refraction.

Now all this reasoning depends upon the assumption that the density of the solar envelope must be approximately 1/140th that of the air in order to bend the ray by the requisite amount. If this assumption be greatly in error, then the whole argument fails. But the ordinary formulas, upon which this assumption is based, for refraction due to a lens, or to the atmosphere, do not apply to the case of a ray of light passing through the solar envelope, as did the rays on the eclipse plates. In the customary optical formulas for lenses, the refractive medium is assumed to be of uniform density throughout, whilst in the cases of the earth's atmosphere and of the solar envelope, the density varies through wide limits, decreasing as the distance from the central body becomes greater and greater. Again the ordinary formula for refraction through the earth's atmosphere is derived upon the assumption that the angle of incidence of the ray remains sensibly constant

while the ray is passing from the highest to the lowest strata of the air. This condition, however, is not filled when a ray passes nearly centrally through a globular mass of varying densities. As such a ray passes from the outer limits of the mass towards the central portion, it meets concentric layers of greater and greater densities and of smaller and smaller radii of curvature. The angle of incidence of the ray, therefore, increases as the ray approaches the centre of the mass and tends to become 90° at the point of nearest approach. At this point the ordinary formula fails completely, for one of the factors increases without limit and makes the refraction apparently enormous. This is shown in Figure 28, where A is a ray of light from the sun or a star as ordinarily observed, and B, a ray

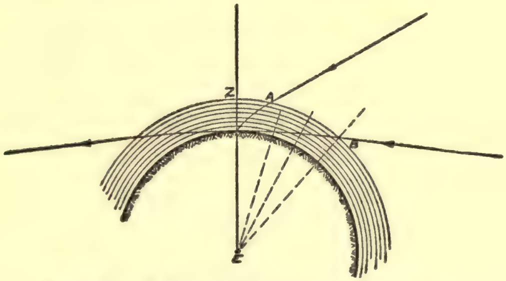


Fig. 28. Failure of Refraction Formulas.

passing through the atmosphere very nearly in a horizontal direction. The ray A meets the successive layers of the atmosphere at nearly the same angle, but the ray B meets the various layers at angles increasing from

about 45°, where it enters the atmosphere, to practically 90° where it grazes the surface of the earth.

Now all the ordinary formulas and theoretical discussions of refraction fit rays similar to A, but fail for rays similar to B. But even for the A rays the theoretical formulas are not wholly satisfactory. The path of the ray depends upon the way in which the atmosphere decreases in density as the height above the surface of the earth increases, and practically nothing is known as to the law of the decrease of density. Near the surface of the earth the atmosphere decreases in density very rapidly, but at higher altitudes it seems to decrease much more slowly. This question of density is involved with that of temperature, and the decrease of temperature with altitude. Various formulas have been derived to express the relation between density, temperature, and altitude, but without complete success. All the formulas fail to fit observed conditions, except very near the surface of the earth. As higher and higher altitudes have been reached by small balloons carrying instruments, the discrepancies of the various formulas have become more and more apparent.

While there is, thus, no satisfactory theoretical formula for refraction, there are tables in common use which give the refraction of a ray with sufficient accuracy for ordinary astronomical purposes. They are extremely accurate for rays which come nearly vertically through the atmosphere, but are little more than

approximations for rays which reach the observer in a horizontal direction, like those from the setting sun.

Now it is matter of common observation that the atmospheric refraction of a horizontal ray is about 35′. The apparent diameter of the sun is slightly less than this, so that, due to this bending of the light rays, the sun is visible for some moments after it has really passed below the horizon. Thus the sun rises earlier and sets later than it would, were the earth's atmosphere removed. Now the total bending of a ray of light, passing entirely through the atmosphere of the earth from side to side and just grazing the surface, would be double the above amount, or approximately 4100″. The maximum observed deflection on the Sobral plates is almost exactly 1″, or 1/4100th that of the earth's atmosphere upon a similar ray. As, under similar conditions, the amount of refraction is proportional to the density of the medium, it would appear that, if the earth's atmosphere were reduced in density to 1/4100th of its normal amount, it would still refract a horizontal ray of light by 1″, the maximum amount of the measured star deflections. This is only about 1/30th of that deemed essential by British astronomers.

Newcomb reinvestigated the theories and formulas of atmospheric refraction, and deduced a formula for the curvature of a horizontal ray, as it passes through the earth's atmosphere. This formula of Newcomb takes into account changes in temperature and pressure,

and gives very satisfactory results for normal conditions of the atmosphere: further, it fits abnormal conditions and gives a clear explanation of mirages and other abnormal phenomena.

Now, if this formula of Newcomb be applied to the case of a ray of light passing through the solar envelope, it becomes apparent, if the formula be applicable, that a very small density will suffice to account for a refraction of 1″; a density many times smaller than has hitherto been deemed essential. But when one thus attempts to reason by analogy from conditions on the earth to conditions near the sun, and to apply formulas derived to fit conditions on the earth it must be done with full reservations as to its validity. The conditions in the solar envelope are so radically different from anything known on the earth, that the application of a formula, which gives consistent results on the earth, may lead one into serious complications. But this applies to the formulas used by the astro-physicists, which appear to show that a high density is necessary, as well as to the formula of Newcomb, which indicates an extremely low density.

This whole question of refraction, even in the earth's atmosphere, is very confused and complicated. Newcomb in 1906 wrote, *"There is, perhaps, no branch of practical astronomy on which so much has been written as on this and which still is in so unsatisfactory a state."*

In view of these different, often conflicting, formulas, with all the complicated and largely unknown conditions in the solar envelope, it is certainly not proved that cosmic refraction is an impossibility.

While, thus, there is a very open question as to the amount of refraction which would be caused by a medium of varying density, there is, on the other hand, practically no question as to the direction in which the bending would take place. This is purely a matter of geometry, and depends upon the fundamental law, that the incident ray, the normal to the surface, and the refracted ray, all lie in the same plane. Provided solely, therefore, that each concentric infinitesimally thin layer is of uniform density and of a geometric shape, the direction in which the ray is refracted can be found by geometrical methods, regardless of the actual density of the layer.

A spherical shell of matter is symmetrical with regard to the centre; the normals at every point of the surface pass through the centre, and thus any and every ray of light passing through such a surface will be refracted radially. From whatever point such a mass of matter be viewed, a ray of light coming through it would appear bent towards the centre. Not so, however, for lenticular masses, or for masses of an ellipsoidal shape. In an ellipsoid the normals from only four points of the surface pass through the centre of figure; and the refraction, in general, will be non-

radial. Only when the eye of the observer is in particular positions with respect to the axes of figure and when the rays of light come through particular parts of the surface will the refraction be radial, or directed towards the centre of figure. But, if the position of the observer, relative to the axes be known, and also the point in which the ray cuts the surface, then from formulas of geometry can be determined the plane in which the refraction takes place, and thence the departure of the deflection from radiality. In the general case of an ellipsoid of matter the formulas become rather long and intricate.

In the case of the photographs taken at Sobral during the eclipse of May 29, 1919, however, an approximate solution may be obtained with great simplicity. For, assuming the solar envelope to be an ellipsoid of revolution with its axis coinciding with that of the sun, the axis of figure would be practically at right angles to the line of sight. On June 3rd this would have been strictly true, and on the day of the eclipse the axis was tilted towards the earth at an angle of only 1°; an angle so small that its effects upon the quantities can be neglected in an approximate solution.

But in order to apply any formulas to the solar envelope, some assumption must be made as to its general size and shape. For the purpose of illustration the major axis of the spheroid may be taken as fifteen times the radius of the sun and the ellipticity as 0.4. The

various values of the departures from radiality, as calculated upon these assumptions, are given in the following table, where for comparison are also given the actual observed values, as determined from the Report of the British astronomers and as shown on the various diagrams of the eclipse plates on page 215.

TABLE V.

Angular Departures from Radiality

Star No.	Observed	Calculated
3	$-3°$	$-4°$
2	$+10°$	$+26°$
4	$+1°$	$-4°$
5	$-4°$	$-25°$
6	$-16°$	$-13°$
10	$-28°$	$-19°$
11	$+36°$	$+24°$

These calculated departures from radiality agree in a striking way with the observed values. It will be noted at once that, with the exception of Star 4, all the calculated departures have the same sign as the observed. The agreement between the calculated and the observed departures from radiality is very good for the four stars, Nos. 3, 6, 10, and 11; the calculated departure for two stars, No. 2, and 5, however, are very much larger than the observed.

In the above calculations the dimensions of the solar ellipsoid were arbitrarily assumed. In order to test the general conclusions, the calculations were repeated

with various ellipsoids, the major axes of which varied from 10 radii of the sun to 18 radii, and the ellipticities from 0.2 to 0.5. In every case the results were the same, the calculated departures from radiality showed a strong general resemblance to the observed departures. Changes in the size of the ellipsoid made very small relative changes in the departures, but changes in the ellipticity produced marked changes in the results. As the ellipticity is increased, the calculated departures in the cases of the outer star images agree more closely with the observed values; but in the case of the inner stars, especially Nos. 2 and 5, smaller ellipticities fit the observations very much better. Further, in all these calculations the axis of revolution of the mass of matter was taken, for simplicity, as being perpendicular to the line of sight. If this axis of figure were tilted towards or away from the observer, then these calculated refractive angles would change; and such tilting would affect the different stars differently.

Now it is not a difficult matter to find by the methods of Least Squares the general shape and position in space of that ellipsoid, which would so refract the rays from the different stars, as to most nearly represent the actual observed deflections (in direction only). The best solution shows the axis to be tilted towards the observer by an amount slightly over 2°. The results for the individual stars are given in the following

table, where they are compared with the observed deflections and with those of Einstein, which, of course, are zero in every case.

TABLE VI

Computed Departures from Radiality

Star No.	Observed	Computed	
		Poor	Einstein
3	− 3°	− 6°	0
2	+ 10°	+ 5°	0
4	+ 1°	− 5°	0
5	− 4°	− 18°	0
6	− 16°	− 11°	0
10	− 28°	− 18°	0
11	+ 36°	+ 33°	0

It needs only a glance at the figures to show how very much better the hypothesis of refraction represents the observed quantities than does the hypothesis of relativity. This comparison of results can also be made by the ordinary method of taking the sum of the squares of the residuals, which method gives,

Relativity theory,	2,489
Refraction hypothesis,	410

The great reduction shown by the refraction hypothesis indicates clearly its superiority over that of Einstein. This is also shown in the following diagram, which represents the eclipse field, and shows for each star the observed deflection, the theoretical Einstein

effect, and the computed refraction effect in direction only.

Further it was found that the ellipsoid which gave the refractive effect most nearly representing the ob-

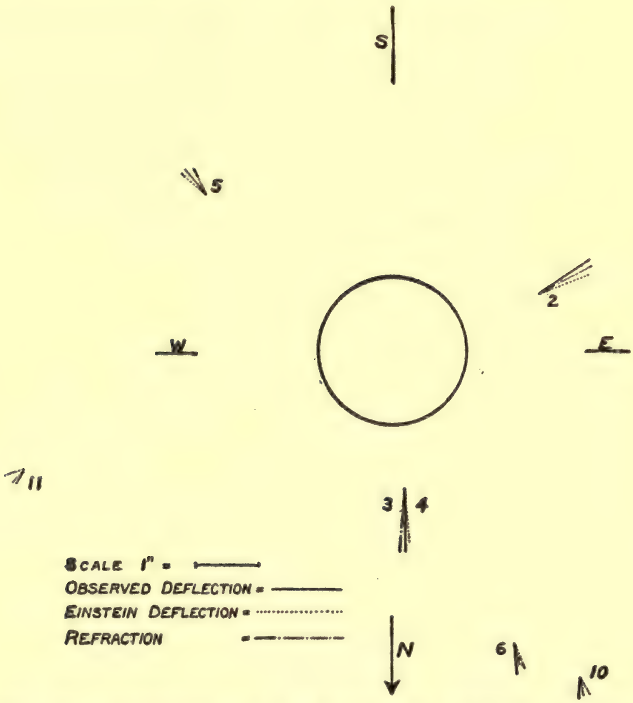


Fig. 29. Comparison of the Einstein and Refraction Effects.

served deflections had its axis tilted in the line of sight. The principal plane of the ellipsoid is, of course, at right angles to this axis, and the position of this plane, in reference to the ecliptic, is thus found to be

Longitude of the Node	44°
Inclination	7°

Now in the discussion of the motions of the planets and of the discordance in the motion of the perihelion of Mercury, the position was found for a ring, or an ellipsoid of matter, within the orbit of Mercury, which would best account for the various motions. This determination was

Longitude of the Node	36°
Inclination	7°.5

Newcomb, in his investigation published in 1895, gave for the values of these quantities

Longitude of the Node	48°
Inclination	9°

This is a striking fact. Two radically different investigations, one on the motions of the planets, the other on the deflections of light rays, both lead to practically the same ellipsoid of matter.

These results indicate, at least, the possibility of explaining the observed light deflections, if such deflections be real, by the refraction of the rays during their passage through the solar envelope, the shape of which is generally that of an oblate spheroid.

CHAPTER IX

CONCLUSIONS

The astronomical evidence, cited by Einstein as complete and satisfactory proof of the relativity theory, fails to support his hypothesis. His hypotheses and formulas are neither *necessary* nor *sufficient* to explain the observed phenomena. They are not *sufficient,* for they account for only one of the numerous discordances in planetary motions, for only a portion of the supposed light deflections: they are not *necessary,* for all the discordances in the motions of the planets, including that of Mercury, can readily be accounted for by simple gravitational methods, and the light deflections, if real, can be equally well explained on other grounds.

A motion of the perihelion of Mercury, similar and approximately equal to that actually observed, can be explained by the Einstein hypothesis. But this hypothesis fails completely to explain other motions of Mercury and similar motions in other planets, it causes new and inexplicable discordances in the motion of Venus. On the other hand, all the observed motions of both Mercury and Venus can readily be explained

by the action, under the Newtonian law of gravitation, of masses of matter, known to exist. And such explanation is based upon formulas and methods, known and used for well over a century to account for similar motions in other portions of the solar system.

Deflections of light rays, similar to those reported at Sobral, can be explained by the relativity theory. This hypothesis can account, very approximately, for the amount of the supposed deflections, but it fails completely to account for the directions in which such deflections occurred. Refraction by the cosmic matter, through which the rays are known to have passed, will account fairly well for the observed directions, but encounters very serious difficulties, in accounting for the amounts of the deflections, as reported.

But for the true relativitist the pathway through all the difficulties of conflicting evidence is smooth and clear; for does not everything depend upon the observer? Nothing is absolute, everything is relative; the statue is golden for one observer and silver to the other. To the relativitist the motion of the perihelion of Mercury, of course, is real and is exactly the 43″ required by the Einstein hypothesis, but the other motions do not exist, they are mere accidental errors. It makes no difference that all these various motions result from the same investigations, that both Leverrier and Newcomb show that the motion of the perihelion is not independent, that it must be accompanied

by and depend upon other motions. These other motions cannot be explained by relativity, and, therefore, they do not exist, they have not been *"sufficiently attested."** Thirty-three photographic plates, taken during the eclipse of 1919, show star images; of these thirty-three, seven only give results even approximating towards the Einstein predictions. And to make even these seven fit the hypothesis, the relativitist is forced to invoke the aid of the sun to distort the camera in a particular way and by just the right amount!

The explanation of the old fashioned astronomer, that the motion of Mercury may be due to masses of matter, which have been seen and photographed many times, is dismissed by the relativitist as having little probability and as having been devised solely for the purpose. The corona of the sun has been known from pre-historic times, the zodiacal light for many years, and meteors have fallen to the earth in all ages. That an elliptical shaped central body would cause a perihelial motion, was shown by Walmsley in 1758, and by him used to explain the motions of Jupiter's satellites. Was this devised solely to explain the motion of Mercury? Did Walmsley devise a method for the sole purpose of explaining something of which he was entirely ignorant, and which was not discovered until nearly a century after his death? The corona, the zodiacal light, meteors, are these fictions of the imagin-

* Albert Einstein: letter of July 30, 1921.

ation? Were these devised by the deluded followers of Newton solely to explain the motions of Mercury?

The relativity theory may be true, but no substantial experimental proofs have yet been submitted by any of its adherents.

APPENDICES

APPENDIX I

THE MICHELSON-MORLEY EXPERIMENT ON ETHER-DRIFT

The Michelson-Morley experiment forms the basis of the relativity theory: Einstein calls it decisive. If it should be shown that this experiment is not decisive, that the negative results obtained were due to instrumental errors or to some peculiar conditions under which the experiments were conducted; if it should develop that there is a measurable ether-drift, then the entire fabric of the relativity theory would collapse like a house of cards. For this reason the repetitions of the Michelson-Morley experiment recently made at Cleveland and at Mount Wilson are of especial importance: they indicate that the original experiment was not decisive, and that there may be a measurable ether-drift.

Many years ago it was suggested that the negative result of the Michelson-Morley experiment might be due to the earth dragging the ether, in its immediate vicinity, along with it: that the ether in the room, in which the experiment was made, was entrapped and

moved with the room. A motor-boat, a steamship, moving through still water drags the particles of water, in immediate contact with its sides, along with it. If one looks directly down from the deck of a moving vessel, one will see the particles of water apparently cling to the sides of the boat and move forward with the boat; particles an inch or two from the surface cling less tenaciously and are slowly passed; particles a foot or two from the sides show no frictional effect and are left at rest by the passing vessel. To measure the true speed of the vessel through the water, one would have to consider the motion of the hull relative to water particles some considerable distance away from the sides of the boat. This effect of dragging water is the well known phenomenon of "skin friction," which plays such an important part in the design of all vessels.

The attempts of Michelson and Morley to measure, in the basement of buildings and at low altitudes, the motion of the earth through the ether might, not inaptly, be compared to the attempts of some minute beings, living in a small rust-pit on the side of the *Leviathan,* to measure the speed of that immense vessel through the waters by experiments in the thin film of water, contained entirely within the hollow in which they lived.

In the years 1891-1897, Sir Oliver Lodge tested this idea of skin friction between moving bodies and the ether, and attempted to measure the amount of such

friction, if any there be. He devised an elaborate apparatus, by which he could test whether the ether contained between two parallel steel plates was dragged along by the plates, when they were whirled at high speed. His experiments showed that the ether between such discs, or plates was not dragged sufficiently to change the velocity of light by so much as the 1/1000th part of the velocity of the plates. And he concluded from this experiment that the viscosity, or fluid friction of the ether is zero. In considering this result it must be remembered that the discs, or plates were only some three feet in diameter, and were placed about one inch apart. The earth is some forty-two million feet in diameter. Thus this attempt of Sir Oliver Lodge to detect possible skin friction of the earth is not radically different from an attempt of a naval architect to find the skin friction of the *Leviathan,* after several months in service, from tests made on a plate of highly polished metal one inch in diameter.

Now the possibility of skin friction between the earth and the ether can be tested by repeating the Michelson-Morley experiment at different distances above the earth's surface. An accurate test, of course, can only be made in the higher regions of the atmosphere, clear above the tops of the highest mountains. This is impossible, but it is possible to utilize high altitude stations and compare the results with those obtained at ordinary levels. This has been done by Professor Dayton C.

Miller at the Mount Wilson Observatory, at an altitude of some 6,000 feet. He there made the experiment with the original apparatus used by Morley and Miller, and repeated it with improved instruments. He summarized his findings in the following words:*

"The suggestion was then made that the earth drags the ether, and while there is no 'drift' at the surface of the earth, it might be perceptible at an elevation above the general surface. The experiment was again performed by the present author at the Mount Wilson Observatory in March and April, 1921, *where the elevation is nearly* 6,000 *feet. The results indicated an effect such as would be produced by a true ether-drift, of about one tenth of the expected amount, but there was also present a periodic effect of half the frequency which could not be explained. The interferometer had been mounted on a steel base and in order to eliminate the possibility of magnetic disturbance, a new apparatus with concrete base and with aluminum supports for the mirrors was constructed. Observations were made in November and December,* 1921, *the results being substantially the same as in April. Before any conclusions can be drawn, it is necessary to determine the cause of the unexplained disturbance."*

These experiments of Professor Miller are not conclusive, but they appear to indicate that the ether is dragged along by the rough surface of the earth, and

* *Science,* No. 1427: May 5, 1922.

Plate 5.

The Ether-Drift Interferometer, with concrete base, as used by Miller in 1921 at Ether Rock.

The original interferometer was made of steel and was subject, therefore, to magnetic effects. The new instrument is made of concrete, with aluminum holders for the mirrors. With this instrument indications of a true ether-drift were found at Mount Wilson.

that the true drift might be measured if one could attain a sufficient height above the surface of the earth. If there be an ether-drift, as these experiments indicate, then the entire structure of the relativity theory is rendered worthless. But, whether there ultimately prove to be a measurable ether-drift at high altitudes or not, this cautious statement of Professor Miller embodies the true scientific spirit, and is in marked contrast to the statements and assertions of the relativitists.

APPENDIX II

EINSTEIN AND THE FIZEAU EXPERIMENT

THE treatment of the Fizeau experiment by Einstein requires a few words of explanation. He gives two equations as follows:

$$W = v + w \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (A).$$

$$W = \frac{v + w}{1 + \frac{vw}{c^2}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (B),$$

in which v is the velocity of the water in the tube, w the velocity of light in a motionless fluid, and W the velocity of light relative to the tube.

He states that equations A and B represent the relations between these quantities, A according to the ordinary theories of classical mechanics, and B according to the relativity theory. He then shows that the relativity equation B more nearly represents the results of Fizeau's observations: *"Experiment decides in favour of equation (B) derived from the theory of relativity, and the agreement is, indeed, very exact"* (48).

Equation (A), however, is not an equation of classi-

cal optics; it is found nowheres, except in Einstein; it has nothing whatsoever to do with Fizeau's experiment. As it stands it is a mere statement that the velocity of light in the moving water is equal to the sum of the velocities of light in air and of the water in the tube. This has never been claimed. Every formula, heretofore used, has involved a quantity that Einstein omits, namely, the index of refraction of water.

Further, the results obtained from equation (B) are not identical with the observational results of Fizeau. In order to bring equation (B) into accord with the results of Fizeau, Einstein is obliged to make approximations, or to neglect certain terms of his own formula. By means of such approximations, he finally puts his equation (B) in the form:

$$W = w + v\left(1 - \frac{1}{n^2}\right),$$

where n is the index of refraction of water, equal to the ratio c/w. And this equation is identical with Fizeau's.

Thus in applying his "crucial test," Einstein sets up and knocks down an equation never before heard of, an equation having no relevancy to the observations discussed, and then adjusts his own equation, by a system of approximations, to fit the observations.

APPENDIX III

THE MATHEMATICS OF RELATIVITY

THE entire relativity theory is based upon certain assumptions, or postulates, from which were derived mathematically all the complicated formulas and conclusions of Einstein. It has always been taken for granted that the mathematics of relativity were correct; that the conclusions followed logically and *inevitably* from the fundamental premises or assumptions. Now this very point has lately been investigated by eminent French mathematicians, especially by Painlevé,* who has shown that a number of different formulas can be derived in the manner of Einstein, that many different and inconsistent conclusions can be drawn from the fundamental premises of relativity.

From his formulas Einstein drew certain conclusions regarding the behavior of clocks and of measuring rods, when in motion. Painlevé has shown that the Einstein formulas are not the only formulas to be derived from the premises, that there is an infinity of

* *Classical Mechanics and the Theory of Relativity, by P. Painlevé. Science Abstracts: Section A—Physics: Vol.* 25, *Part* 3, *page* 170. *March* 31, 1922.

other possible formulas. One of these other possible formulas leads to the ordinary results of Euclidean space and to the constancy of rigid bodies. Other possible formulas lead to the conclusion that bodies expand instead of contracting, still others that they expand at right angles to the direction of motion.

The conclusions of Einstein appear to Painlevé to be audacious conjectures and not the *inevitable* consequences of the premises: he concludes that it is pure imagination to pretend to draw conclusions such as Einstein does. He believes that a number of Einstein's formulas will blend with classical science, but that some of the more startling consequences of the theory will not finally survive.

APPENDIX IV

THE DISPLACEMENT OF SOLAR LINES AND RELATIVITY

EINSTEIN has claimed that the observations of Grebe and Bachem at Bonn on the cyanogen lines in the solar spectrum place the reality of the relativity displacement almost beyond doubt, and in these observations he sees clear experimental confirmation of his entire theory. It has been noted, however, in Chapter II that the bands or lines of the solar spectrum are subject to displacements due to other causes, to motions of the earth and sun, to motions of the solar atmosphere, and to differences of pressure. These displacements may be much larger than the predicted Einstein effect. Thus, the relativity, or Einstein, shift is not a clear-cut effect which can be directly measured; it must be disentangled, if it exists, from several similar, overlapping, and even larger effects.

In the Annual Report of the Director of the Mount Wilson Observatory of California is to be found a summary of the observations upon the cyanogen lines, made by various observers, each of whom claims to have

proved the existence of the Einstein effect. From this summary, the following facts appear:

PEROT applied corrections for *downward* movement of the solar atmosphere and for *negative* pressure shift (approximately equal to the Einstein shift), and when thus corrected, his results agreed with the Einstein prediction.

BIRGE applied a correction for an *upward* movement of the atmosphere, but *no* pressure shift. and when thus corrected, his results agreed with the Einstein prediction.

GREBE and BACHEM assumed neither *upward nor downward* movement of the atmosphere and *no* pressure shift, but applied a correction for a supposed *asymmetry* of the arc-lines, and, when thus corrected, their results agreed with the Einstein prediction.

Had these three observers applied the same corrections, in the same way, it is perfectly clear that their final results would have been very discordant, and that two sets of results, at least, would have differed radically from the predicted Einstein effect. As a matter of fact, these three sets of observations, taken together, do not show the slightest trace of the relativity effect; they are radically discordant and can only be made to show the desired result by arbitrary and contradictory corrections.

Mr. St. John, of the Mount Wilson Observatory, sums up the various observations, in the Annual Report of the Director, in the following words:*

"Owing to the different and even inconsistent corrections applied to the observed sun-arc displacements, the resulting approximate agreement with the deductions from the Einstein theory fails to carry conviction."

This statement is certainly conservative!

* Carnegie Institution of Washington. Annual Report of the Director of the Mount Wilson Observatory. Year Book, No. 20, for the year 1921, p. 244.

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